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HEMORRHAGE FROM THE STOMACH AND DUODENUM

BY WILLIAM J. MAYO, M. D., ROCHESTER, MINNESOTA

GIVEN a patient who has suffered from gastric distress, acidity, and other evidences of ulcer, and then suffers from repeated hemorrhages shown in the vomitus and in the stool, the diagnostic value of the hemorrhage is very great; but a single hemorrhage from a patient who has not had previous gastric symptoms is probably not due to ulcer.

Hilton Fagge long ago called attention to rupture of the veins about the cardia from cirrhosis of the liver and also to swallowed blood from hæmoptysis, as a frequent cause of supposed gastric hemorrhage. With increasing experience we are less inclined to look upon hemorrhage as either a sure sign of, or its absence as contraindicating, the presence of ulcer.

An examination of the gross specimen of the contents of the stomach after the test meal for blood is important, and sometimes of even greater differential value than the finer laboratory tests, especially as between ulcer and cancer. The delicate tests now used may sometimes give a reaction from blood produced by the slight traumatism inflicted by the introduction of the stomach tube itself, which an examination of the whole specimen would show to be traumatic.

Visible or occult blood in the stool affords proof as to the fact that there is blood, but it should never be lost sight of that it bears with it no evidence as to it exact gastrointestinal origin. The patient may have bleeding gums or hemorrhage from some slight abrasion in any part of the many feet of mucous membrane which exist between the lips and the anus. If occult blood is found by one chemical test, it must be corroborated by others, as some unsuspected food or drug may give rise to the reaction.

As a matter of fact, hemorrhage from ulcer is by no means of frequent occurrence. The base of the ulcer is clean and free from granulation tissue so that bleeding may be infrequent. Careful examination of the stool for many days may be necessary to detect its presence; quite contrary to the condition in cancer, where occult blood in the fæces will be found in the majority of cases, in this respect resembling the hemorrhage from cancer of the cervix, rectum, bladder, mouth and other mucous surfaces.

Graham says that "Coffee ground blood and lactic acid in the stomach contents are strong indications of cancer, and that occult blood will usually be found in the fæces of these cases, especially, if the free hydrochloric acid is reduced or absent."

Copious hemorrhage at infrequent intervals is the history of a considerable percentage of ulcers, while continuous small hemorrhages are the rule in cancer, although in some cases continuous loss of small quantities of blood does take place in ulcer, producing a degree of anæmia which requires careful differential blood examination to tell from the pernicious type, and occasionally a cancerous erosion of large blood-vessels will cause death from hemorrhage.

Ninety to ninety-six per cent of gastric and

duodenal hemorrhages cease spontaneously. Fatal hemorrhage occurs in from four to ten per cent, and in fifty-five per cent of these the splenic vessel is involved. (Brinton.)

Eighteen and one-half per cent of those dying from hemorrhage die so quickly that an operation could not be performed. (Savariaud.)

Bleeding ulcers can be divided into three groups: First, the acute round peptic ulcer, of Cruveilhier. In several cases of this type, which we have had an opportunity to examine, where the stomach was opened and the bleeding point found, the ulcer did not prove to be round, but rather a small fissure which was detected with difficulty.

The second variety is the mucous erosion of Dieulafoy varying to a greater or less extent in size, and in which the affected mucosa "weeps" blood. Like the acute peptic ulcer mucous erosion is probably infective or toxic in origin. Fig. 1, is a photograph of the gastric mucous membrane in a fatal case of the Dieulafoy ulcer which came under our observation.

In the older literature on this subject we find constant reference to the peculiar frequency of gastric hemorrhage in young women, which are of doubtful origin, but supposed to be due to ulcer. Hale White described some cases of hemorrhage of the stomach in young women which belong to this category and which he calls gastrotaxis.

Acute peptic ulcer and mucous erosions are rare conditions. Generally speaking, hemorrhage from these acute processes belongs to the

Fig. 1. Photograph of large mucous erosion of stomach.

Fig. 2. Saddle ulcer of lesser curvature involving anterior and posterior surfaces, showing varicose blood vessels in the margins of the ulcer. Anterior margin of excision outlined by dotted lines.

domain of internal medicine rather than that of surgery, and a large percentage of these patients recover spontaneously.

The third variety, that of hemorrhage from a chronic ulcer, is surgical, although not necessarily operative during the acute stage. (Rodman.) The whole thing turns on the history of the patient. If the patient gives an ulcer history extending over a period of time before the hemorrhage, this at once differentiates it sharply from all the non-surgical hemorrhages of the stomach, and in the large majority of cases, from these little understood processes which are included with acute peptic ulcers and mucous erosions.

More than ninety per cent of hemorrhages from the stomach are from chronic ulcers with a well-marked ulcer history. In the greater number the condition would have warranted operation independent of the hemorrhage. So long as the surgical indication furnished by the hemorrhage is backed by the ulcer history, we know that we have a definite local lesion to deal with, and that the patient will be relieved not only of the hemorrhages, but also from the chronic disability of the primiary disease.

I do not mean to say that operation should not be practiced where there is hemorrhage from the stomach without previous history of ulcer, but rather that in such cases the burden of proof must show why an operation should

be performed; while in hemorrhage from chronic ulcer, the burden of proof must show why an operation should not be performed.

As to the surgical treatment of hemorrhage, there is some difference of opinion. Many surgeons believe that gastrojejunostomy is efficient as a curative agent without direct attack upon the bleeding vessel. While we would agree with this opinion as regards small and moderate hemorrhages, in cases of severe bleeding from prepyloric and duodenal ulcers, gastrojejunostomy may prove inefficient. We had one patient die from hemorrhage twelve days after gastrojejunostomy; and post-mortem revealed the open vessel in the margin of the ulcer. A second case of the same nature required re-operation with direct attack upon the bleeding vessel in the ulcer six days after gastrojejunostomy. A third suffered repeated hemorrhage following and two years later the ulcer was excised with cure. Many such experiences are recorded in literature.

Gastrojejunostomy after the posterior "no loop" method gives ideal results if there is actually or potentially, interference with the progress of food. When the ulcer is some distance above the pylorus permitting of ready egress by the normal outlet, gastrojejunostomy is of less value.

There has been considerable discussion as to whether the bowel in gastrojejunostomy should be turned to the right or allowed to remain in its normal position to the left. Regarding this point I can only say that in 292 cases of "no loop" gastrojejunostomies in which the intestine was sutured as it runs to the left, we had but three deaths and two re-operations; both of the latter from adhesions which were corrected at the second operation by fastening the bowel to the left beyond the gastrojejunostomy to prevent angulation.

In connection with the gastrojejunostomy, if an ulcer exists in the stomach it should be excised if possible. When this is not feasable

Fig. 3. Showing closure of stomach defect after excision of ulcer in such manner as to enlarge rather than diminish the lumen. Anterior half of suture line shown.

the main blood-vessels leading into it should be ligated and the peritoneum and muscular coats drawn over it. Excision, however, is the better procedure as it gets rid of the disease and prevents a possible secondary cancerous degeneration. (Figs. 2 and 3.)

Bleeding ulcers which lie a considerable distance above the pylorus should be excised. In extensive ulcers, such as the hour-glass variety, this may amount to resection in continuity. In doing this resection much deformity may result from the plastic closure of the gap produced by removal of the ulcer, and should the deformity interfere materially with drainage through the pylorus, gastrojejunostomy n addition may be required. In hemorrhage from duodenal ulcer, ligation of the blood vessels and closure of the outer coats over the indurated area with gastrojejunostomy, will be found efficient.

In the majority of bleeding ulcers, both of the stomach and duodenum, the blood vessels leading into them are varicosed. (See Fig. 2.)

For severe hemorrhage from the stomach, in those cases in which no ulcer can be located from the exterior, we have followed a modification of the Andrews-Moullin method, opening the anterior wall of the stomach by a longitudinal incision, and by counter pressure cause successive areas of the mucous membrane to present itself at the opening until the bleeding point is detected. With chromic catgut on a small curved needle, the hemorrhagic area is sutured from the mucous side. Over this, on the peritoneal side, a few linen sutures are introduced for protection, after which the working incision into the stomach is closed in the usual manner.

FALSE DIVERTICULA OF THE APPENDIX

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IT is well known that the appendix is subject to various affections, mainly of an inflammatory nature, which may be attributed to peculiarities in its anatomical structure.

The large amount of lymphoid tissue, lying between the muscular and mucous coats, its dependent position at that part of the intestinal tube where the bacterial flora attains its greatest luxuriance, a somewhat precarious blood supply, a mobility allowing of changes in position which may readily interfere with the outflow from so small and narrow a tube — all these are factors peculiar to the appendix, which is thus rendered a prey to grave infective processes far more frequently than any other division of the intestine.

Apart from these diseases, however, the appendix shares with other parts of the alimentary tract a liability to become the seat of new growths, or to be the starting point of certain chronic inflammatory diseases such as tubercular or actinomycotic infections.

These are all well known; and now that the appendix is more often submitted to microscopical examination, so are cases exemplifying such conditions reported with greater frequency.

There is another pathological change occasionally met with in the intestine, particularly the large bowel, which may also occur in the appendix; this is the mucous hernia or false diverticulum. Attention was called to this affection by Seelig in an article published in the Annals of Surgery,[1] and as an example came under my observation about that time I was led to investigate the subject, thinking that the condition might be less rare than the paucity of reported cases suggested. More recently a striking case has been reported by Herb,[2] and in this article will be found references to the literature on the subject.

The material I found available for investigation comprised some three or four hundred

[1] 1906, xliv, 78.
[2] Journ. Amer. Med. Assoc., 1907, xlix, 2135.

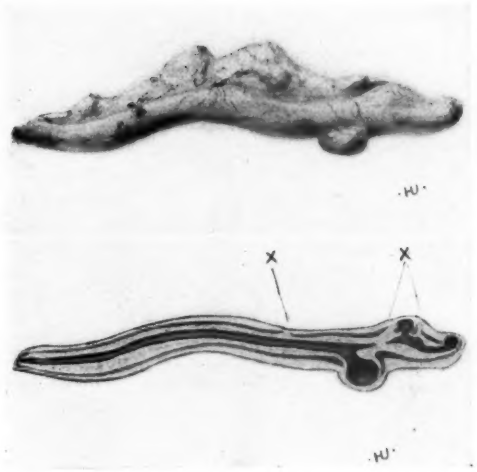

Fig. 1. Appendix in Case 1. Shewing diverticulum on antimesenteric surface, one inch from top.
Fig. 2. Longitudinal section, Case 1. Shewing three diverticula II gaps in muscle coat.

appendices, the exact number is immaterial since they were not all examined by section or with the microscope and in the absence of such methods it is quite possible that several examples may have been overlooked. Among these were two well-marked instances of diverticula formation, one of which had been recognised at the time of operation, and several specimens which seemed to throw some light on the possible causation of the condition.

For permission to make use of their cases, I am indebted to Mr. H. Littlewood and Mr. B. G. A. Moynihan, who kindly afforded me access to the appendices removed by them, both from cases under their care in the Leeds General Infirmary and from private patients. A most excellent example has been sent to me by Mr. J. F. Dobson, of Leeds. (Figs. 10 and 11.)

Case 1. This appendix (Fig. 1.) was taken from a woman, aged about 50, and although she had complained of indefinite pains in the right side of the abdomen for some time, yet there was no history of a severe attack, such as might be caused by a perforation. The organ is somewhat lengthy; it presents about one inch from its tip, on the side opposite its mesentery, a hemispherical projection, the size of a pea. The surface of the appendix, near this swelling is slightly injected, the appearance of the remainder of the organ does not depart in any marked degree from the normal, though towards its cæcal end it is considerably narrower than in its distal portion.

On laying open the appendix (Fig. 2), the line of section falling across the diverticulum, the gross changes are clearly seen. The lumen of the proximal half of the tube was obliterated with inspissated debris; the tip of the appendix is distended and protrudes through the muscular coat, while through a hiatus in this layer, one inch from the tip, a well marked hernia of the mucosa is apparent, filled with cellular debris. Beyond this comparatively large diverticulum are two minute protrusions, each appearing on the surface of the appendix as a small nodule, about the size of a hemp seed. (Only one of these smaller herniæ is seen cut across in the figure.)

Microscopical sections of the larger diverticulum shew a remarkably abrupt ending of the muscular coat of the appendix on either side of the gap, through which the mucosa has bulged. The epithelial lining of the diverticulum is eroded, the cells are shorter and the tubules scarcer than usual. The muscularis mucosæ can be traced around the diverticulum.

Case 2. The history of the case from which this appendix was obtained, could not be traced. This organ is represented diagrammatically in Fig. 3, which also shews the lumen and the diverticula therefrom. Transverse sections, as seen under a low magnification, are depicted in Figs. 4, 5, 6, and 7.

Towards the distal part of the appendix, on the side away from the mesentery, was a small projection which aroused my interest; the layers of the meso-appendix were separated by what I took to be considerable glandular enlargement. On cutting across the appendix, the swelling on the anti-mesenteric surface was seen to be due to a diverticulum, while the swellings which still appeared to the naked eye as swollen glands in the mesentery, proved on microscopical examination to be other diverticula filled with debris. By reconstruction of the appendix from these sections, it was found that it presented four diverticula, three of which protruded between the layers of the mesentery.

It is this latter fact which makes it seem probable that other examples of this condition were overlooked, for a swelling of the mesentery, presumably due to in-

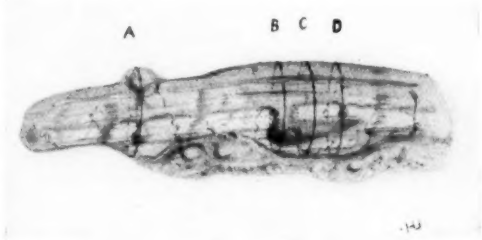

Fig. 3. Schematic reconstruction of appendix, Case 2. Shewing lumen with four diverticula. A, B, C, D shew position of sections in following figures.

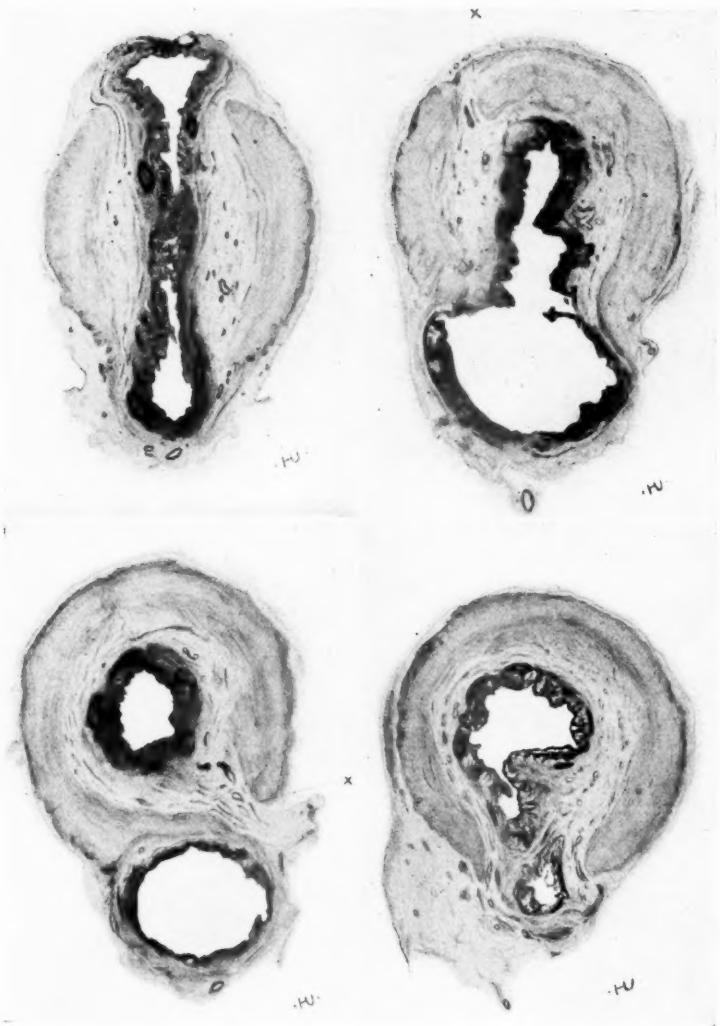

Fig. 4. Transverse section of appendix, Case 2. Section passes through two diverticula on opposite sides of appendix.
Fig. 5. Section of appendix, Case 2. (Fig. 3B.) X shews gap in muscle coat with vessels passing through.
Fig. 6. Transverse section of appendix, Case 2. (Fig. 3C.) Passes through same diverticula as Fig. 5.
Fig. 7. Transverse section of appendix, Case 2, (Fig. 3D.) It passes through tip of diverticulum seen in Figs. 5 and 6, and also through a smaller hernia protruding along side vessels.

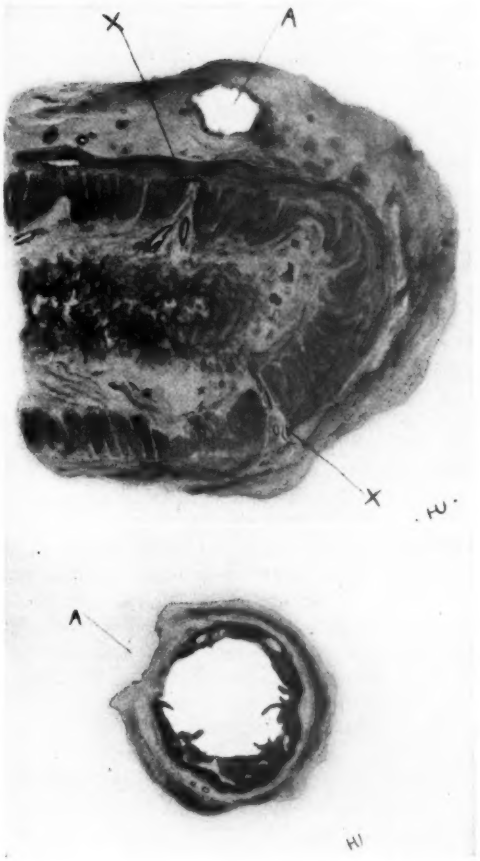

Fig. 8. Section through tip of appendix shewing gaps in muscle layer through which vessels are passing. A, a minute focus of suppuration in subperitoneal tissue.

Fig. 9. Section of perforated appendix. A, floor of ulcers formed only by mucous and sub-mucous coats.

flammatory œdema, was not deemed sufficient reason to lead to further and more minute investigation.

Two of the protrusions in this specimen occur opposite each other and are included in one transverse section (Fig. 4). Another diverticulum has insinuated itself for a short distance between the layers of the meso-appendix parallel with the lumen of the tube, while the fourth is a minute hernia not yet passed beyond the muscular envelope.

Case 3. This specimen was removed from a man who had had two attacks of appendicitis. The appendix is short and thickened, the opening into the cæcum is impervious and the organ was removed by Deaver's method with a small portion of the cæcum. The proximal part of the appendix is distended, and it presents on the side away from the mesentery and quite close to its base a thin-walled, almost translucent diverticulum a quarter of an inch in diameter. Adherent to the mouth of the diverticulum and partly occluding its opening is one end of a fibrinous strand which passes obliquely across the lumen of the appendix. A small tag of omentum is adherent to the outer surface of the diverticulum. (Figs. 10 and 11.)

Diverticula of the intestine may be divided into two classes, congenital and acquired. The walls of the former are composed of all the coats of the bowel, they are, therefore, true diverticula; of these the best example is afforded by Meckel's diverticulum. Probably also those small blind protrusions of the upper part of the small intestine, commonly associated with aberrant portions of pancreatic tissue, should be included under this heading, though by some they are regarded as acquired true diverticula, resulting from traction.

Acquired diverticula may be either true or

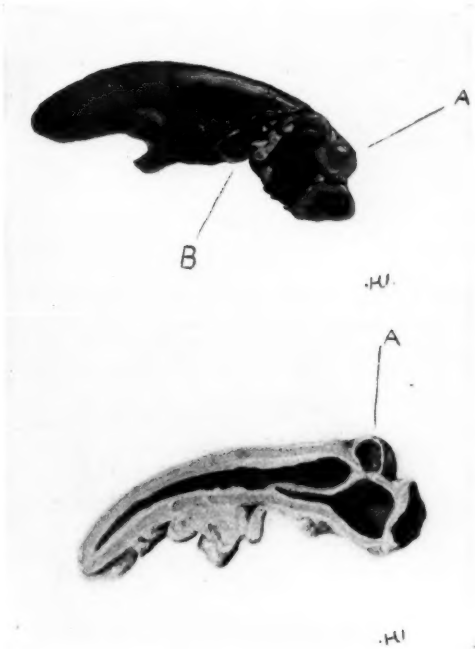

Fig. 10. Appendix; Case 3. A, diverticulum. B, adherent omentum.

Fig. 11. Section of appendix from Case 3, shewing diverticulum at A.

false, and it is to the latter class the specimens I have described must be referred.

It will be interesting to turn for a moment to false diverticula, as found elsewhere in the intestine, in the hope of throwing some light upon their causation.

They have been found in the small intestine as multiple protrusions, occurring between the leaves of the mesentery; and it has been shewn that in their course they appear to follow the terminal branches of the mesenteric vessels. Whether undue distension of the bowel is necessary for their development is not certain, but their choice of position is probably determined by the fact that the perivascular lymph space affords but little impediment to the early stages of their development.

False diverticula are most frequently found in the large bowel, the ilio-pelvic colon being the seat of election. In this situation they may give rise to conditions of the utmost importance from the surgical point of view. Whether these diverticula of the large intestine follow the same rule in protruding opposite the blood-vessels is a debatable point; one such specimen that I have examined gave me no reason for thinking that this was the case.

To return to the appendix: there are one or two points in its anatomy and certain pathological changes relative to its perforation by infective processes which are of importance in this connection.

No other portion of the human digestive tube has a muscular coat so thick in proportion to its lumen; piercing the thickness of this coat, are gaps or spaces through which blood-vessels enter and lymph-channels emerge from the sub-mucous layer. One such gap or hiatus is described by Lockwood [1] as being constantly present opposite the base of the meso-appendix, but I have found these gaps not confined to the base of the organ. Fig. 8 is a drawing of a section through the tip of an appendix, and it shews several of these gaps in the muscular layer. Figs. 2, 5, and 6 illustrate the same point.

Possessed of this stout contractile envelope, the appendix must be capable of exerting considerable pressure upon its contents when for any reason its outlet becomes obstructed. It is possible that under such conditions, if long continued, the mucosa may become herniated through one of the spaces which I have mentioned, though it is doubtful if this ever takes place unless the gaps are rendered more permeable by disease. A closer examination of those appendices which are the seat of diverticula shews that their muscular coats are thicker than usual, the blood-vessels appear to be not only particularly numerous but also possess greatly thickened walls; moreover the sub-mucous meshwork wherein they lie is of greater extent than normal, with the naked eye the ring of mucosa is seen to be separated by a wide interval from the investing muscular layer. Under the microscope the sub-mucosa is seen to contain many new connective tissue elements and several of the thick-walled vessels are occluded by organised clot. All these appearances I take to indicate a former period of inflammation through which the appendix has passed; the effective agents of the inflammation have been subdued only after a long period of reaction on the part of the tissues; the effects of this reaction to a long continued stimulus have resulted in an over-production on the part of the mesoblastic tissues, in other words, a productive type of chronic inflammation.

The result of this hyperplasia of the muscle coat will be to increase the pressure upon its contents; at the same time the weak points in this layer will be still further weakened by the increased size of the vessels traversing them, and by the accompanying perivascular tissue. Under such conditions as these a hernia might well protrude itself through one of these spots of lessened resistance. The appearances seen in sections of appendix 2 (Fig. 4, 5, 6, and 7.) lend support to this view.

Thus far no mention has been made of the relation of perforation to diverticula formation. That a diverticulum may lead to a perforation, indeed favour its occurrence, will be readily admitted, and if the protrusion is not situated between the leaves of the meso-appendix so much the greater will the danger be. Specimens demonstrating this sequence of events are not easy to find, for the infective gangrene, the essential factor in perforation, so destroys the relations of parts that their previous condition can be only surmised.

[1] Appendicitis: Its Surgery and Pathology. Macmillan.

Whether the reverse order of events may occur, a perforation lead to a subsequent mucous hernia, is hardly susceptible of proof from the limited number of specimens examined.

It should be remembered that neither in the case where I was able to record the anamnesis nor in Seelig's case were there any clinical phenomena indicative of former perforation. Still there are two ways in which a perforation might lead to a diverticulum, in their pathogenesis they may be likened to traumatic aneurisms. In one case a wound of the vessel heals and the resulting scar gradually gives way at some later time. In the other case, the vessel walls are weakened by a local destruction of their outer coats and at this weakened spot the inner coat protrudes.

A stage in the latter process may perhaps be illustrated by Fig. 9, taken from an appendix which presented a small clean cut perforation not obscured by swelling of the tissues around. The section does not actually pass through the perforation, but is extremely close to it. It can be seen that, for a portion of its circumference, the wall of the appendix is formed of mucous membrane and submucosa alone; on either side of this area the circular and longitudinal muscular coats end abruptly. The edges of the muscular layer are slightly swollen and, for a short distance away from the edge, the nuclei fail to take up the stain. That part of the submucosa which forms the floor of the deficiency is also devoid of staining nuclei. Except for chronic changes in the mucosa, the remainder of the circumference of the appendix appears fairly normal.

The inflammatory process appears at one point to have passed the restraining barrier of the mucous membrane without arousing the resisting forces of the leucocytes, and, finding the tissues unprepared for defence, the bacterial toxines have lead to a rapid death of the highly specialized muscle cells, and, to a more limited extent, of the sub-mucous tissue.

Why the destruction should be so limited in the absence of any marked protective reaction is not quite clear; it may be that it is due to a local diffusion of bacterial poisons alone, the organisms giving rise to them falling in the breach as it were. If it is permissible to imagine such a breach of microscopical extent, followed by a limited and finite destruction of the muscular coats, it is easy to see how a false diverticulum may arise from any later increase in intra-appendicular pressure.

In Seelig's case there was an absence of muscularis mucosæ, both in the wall of the appendix and of the diverticulum; and he quotes Obendorfer as stating that this is evidence of past inflammation. I believe that an inflammation needs to be servere that it may destroy this membrane completely; in milder infections it is easy to see that the muscularis mucosæ is destroyed only opposite the swollen and infiltrated lymphoid follicles. In the specimens which I report, the muscularis mucosæ can be traced in the wall around the greater part of the diverticula.

In view of the small number of specimens recorded, it is not possible to make any definite statements as to the etiology of the condition. It seems probable, however, that such appendices have always been the seat of a chronic inflammation, that this has the effect of increasing the relative size of the gaps in the muscle coat through which the vessels enter, and that it is through these gaps that the hernia are extruded.

That a diverticulum may follow an acute infective necrosis limited to a small portion of the appendix wall is not proved by the material I have examined, though certain specimens of perforation are suggestive of its occurrence.

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Why the destruction should be so limited in the absence of any marked protective reaction is not quite clear; it may be that it is due to a local diffusion of bacterial poisons alone, the organisms giving rise to them falling in the breach as it were. If it is permissible to imagine such a breach of microscopical extent, followed by a limited and finite destruction of the muscular coats, it is easy to see how a false diverticulum may arise from any later increase in intra-appendicular pressure.

In Seelig's case there was an absence of muscularis mucosæ, both in the wall of the appendix and of the diverticulum; and he quotes Obendorfer as stating that this is evidence of past inflammation. I believe that an inflammation needs to be servere that it may destroy this membrane completely; in milder infections it is easy to see that the muscularis mucosæ is destroyed only opposite the swollen and infiltrated lymphoid follicles. In the specimens which I report, the muscularis mucosæ can be traced in the wall around the greater part of the diverticula.

In view of the small number of specimens recorded, it is not possible to make any definite statements as to the etiology of the condition. It seems probable, however, that such appendices have always been the seat of a chronic inflammation, that this has the effect of increasing the relative size of the gaps in the muscle coat through which the vessels enter, and that it is through these gaps that the hernia are extruded.

That a diverticulum may follow an acute infective necrosis limited to a small portion of the appendix wall is not proved by the material I have examined, though certain specimens of perforation are suggestive of its occurrence.

PRIMARY OVARIAN PREGNANCY[1]

With the Report of a Case

By C. C. NORRIS AND C. B. MITCHELL

OVARIAN pregnancy is one of the rarest of all forms of extrauterine gestation, so little of the condition is known that the greatest interest is attached to each new case recorded. The term ovarian pregnancy should be applied only to those cases in which the ovum has been fertilized before its escape from the Graafian follicle, the spermatozoa having directly entered the follicle just after its rupture. This may also occur in a deeper follicle, which has ruptured into a more superficial one, the spermatozoa passing through this to the ovum, lying in the deeper follicle. Although there is no longer any doubt that such a fertilization may occur with a resulting pregnancy in the ovary, the rarity of the condition demands a careful report of each case. Dr. Howard Kelly considers this one of the greatest of all gynecological rarities and has never had a case. The thought has been advanced that the condition occurs more frequently than has been supposed and that a careful examination of all cases of ovarian tumors would greatly increase the percentage of cases. This in a certain measure is true; nearly as many cases having been reported during the last six years as were reported during the entire previous century. Yet it must be granted that the condition is still a very rare one. In our laboratory of Gynecological Pathology at the University of Pennsylvania where, during the last 8 years, some 1700 gynecological specimens have been examined, we have had but one case of ovarian pregnancy. During this time we have examined 58 hemorrhagic cysts of the ovary, 62 small ovarian tumors including dermoids, 44 extrauterine pregnancies — all of which conditions are supposed to be occasionally mistaken for ovarian gestations. Among this number were two cases which macroscopically closely simulated ovarian pregnancy; but a careful histological study of serial sections from the tube proved one to be a case of tubal abortion and the other a tubo-ovarian pregnancy. Such cases, however, without a thorough histological examination might easily be mistaken for ovarian pregnancies.

The discussion as to the possibilities of the occurrence of ovarian pregnancy, extended from the 17th century to 1901. So many cases were reported which were undoubtedly false that the scepticism of many authorities is not to be wondered at. No definite criterion had been formulated on which a diagnosis could be based; and until Spiegelberg in 1878 laid down the following conditions, which should be fulfilled in making a diagnosis, no really accurate work on this subject had been done.

1. The tube on the affected side must be intact and have no organic connection with the gestation sac.

2. The fetal sac must occupy the position of the ovary.

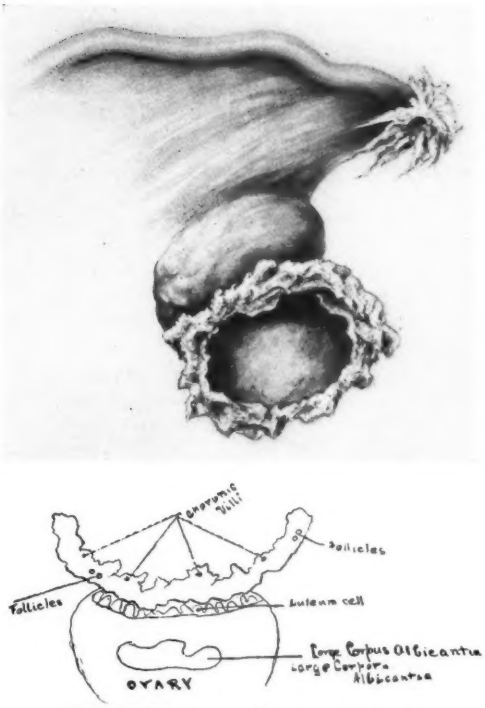
3. It must be connected with the uterus by the ovarian ligament.

4. Definite ovarian tissue must be found in the sac-wall in several places.

German authorities have for a long time admitted the possibility of ovarian pregnancy; and it is from them that we have some of the most carefully worked out studies of this condition. In 1898 Dr. Catherine van Tussenbroeck, of Amsterdam, reported a case of ovarian pregnancy before the Third Obstetrical and Gynecological Congress. This case undoubtedly did more to establish the fact that such a condition might exist than any other work. However, English scientists led by Bland-Sutton, remained unconvinced until 1901, at which time Bland-Sutton went to Amsterdam for the special purpose of examining von Tussenbroeck's case and after a careful study, admitted the correctness of her first report. Shortly after this time there appeared in America the very careful reports of Thompson and Webster.

Classifying the cases under the heads sug-

[1] Read before the Philadelphia Obstetrical Society, January 2, 1907.



Drawing of specimen with schematic sketch.

gested by Williams, we have found up to the present time 16 positive cases, 15 probable cases and 9 fairly probable which are as follows:

Positive Gottschalk	Highly probable	Fairly probable
van Tussenbroeck & Kowwer	Granville	Peuch
Anning & Littlewood	Hein	Geuer
Francz	Uhde	Kustner
Thompson	Franck	Toth
Robson	Schrenck	Croft
Hewetson	Walther	Guilford
Scheckele	Spiegelberg	Michanx
Boesebuch	Herzfeld	Frank
Bandel	Larsen	Law
Micholitsch	Leapold	
Kantorsvicz	Martin	
Webster	Gottschalk	
Kelley, J. K.	Lugwig	
Herrenschmidt	Czygwicz	
Freund	Oliver	

All of the positive cases are of about three months or shorter duration; 12 of the highly probable cases are at term, which is interesting in illustrating the greater distensibility of the ovary over that of the tube, a much small number of tubal pregnancies developing to full term before rupture. It is, however, readily seen that it is in the earliest cases that the most accurate diagnosis can be made. The great growth of the fetus destroys the relations and obliterates normal tissue, to such an extent that the fulfillment of all of Speigelberg's criteria is quite difficult.

The cases of von Tussenbroeck, Kelly, and Hewetson show the fully developed corpus luteum in relation to the fetal sac. In the case of Webster no such relation was noted.

It is interesting here to consider the theory of Webster, that the presence of Müllerian tissue is an essential factor for the developing of a fertilized ovum. The uterus and Fallopian tubes both develop from this tissue but the ovary arises from the Wolfian body. Therefore, according to Webster's theory, there must have been an embryonal inclusion of Müllerian tissue in an ovary which is destined ever to be the seat of a pregnancy. This theory is even of greater interest in considering the possibility of primary abdominal pregnancies.

PATHOLOGICAL REPORT

Name, Mrs. B.; patholog. No., 1684; operation, right salpingo-oophorectomy.

PATHOLOGICAL DIAGNOSIS

Normal tube. Ovarian pregnancy (primary).

MACROSCOPIC DESCRIPTION

The specimen consists of a tube and ovary.

Tube. The tube is normal in shape and size, and has the following measurements: It is 8.25 cm. in length. The diameter at the uterine extremity is 5 mm.; at the middle of the isthmus it is 6.5 mm.; and through the ampulla it is 8 mm. The surface is free of adhesions. The abdominal ostium is open. The fimbria are luxuriant. On section, the walls are normal in thickness and consistency. The lumen is empty. The mesosalpinx is normal.

Ovary. Measures 2.80 x 1.60 x 1.40 cm. Its surface, especially the posterior aspect shows numerous, vascular adhesions. The tunica albuginia is white and glistening. Attached to, or springing from the anterior and inferior surface, is a portion of the wall of a hemorrhagic sac. The outer surface of this is covered by a thin membrane, attached to which are a few vascular adhesions. On the inner surface, and making up the chief substance of the wall of the sac, is what appears to be an organized blood-clot. The sac shows a rounded outline, the concavity being away from the ovary. It measures 4 x 5 cm., and from the

appearance of the specimen it seems probable that the entire sac measured about 8 cm. in diameter. The inner surface of the sac shows numerous small dome-like elevations. Attached to these, and more or less over the entire inner surface, are shreds of whitish tissue, which upon close examination are found to be made up delicate thread-like projections which vary from 2 to 4 mm. in length. These are quite strong. This tissue resembles young placental tissue. The wall of the sac varies from 4 mm. to 1 cm. in thickness. The edges are rough.

HISTOLOGICAL DESCRIPTION

Tube. Serial sections from the outer third of the tube, including the fimbria, show this portion of the organ to be entirely normal.

Ovary and Gestation Sac. The section is through the entire thickness of the sac and almost the entire ovary.

The outer covering of the sac is made up of one or two layers of flattened cells which have deeply staining nuclei. Beneath this is a thick layer of organized blood-clots in which at various points can be distinguished chorionic villi, in some of which trophoblastic cells can be distinguished. The villi appear somewhat degenerated. Numerous syncytium cells can be distinguished. Some of these are arranged in a double chain of cells while others appear singly or in groups. Chorionic villi giant cells are seen.

The hilum of the ovary is rather more vascular than usual, but otherwise it is normal. The substance of the organ also appears unusually vascular. A number of corpora albicantia are present. One of these is unusually large, occupying about the third of the entire bulk of the ovary. Between this and the gestation already described, is a corpus luteum, from the depression on the surface of which the gestation sac has its origin. The corpus luteum covers the entire base of the gestation sac and in one area extends slightly beyond it. The lutein cells are in this case easily recognizable. The ruffle or fan-like arrangement so often spoken of in describing the yellow body is well shown. Some of the cells contain pigment. The differentiation of the individual lutein from the decidual cell is impossible; but from the arrangement of these cells and the general appearance of the sac which they line, we feel justifiied is stating positively that these are lutein and not decidual cells. Immediately beneath the sac the layer of lutein cells is thin but unmistakable, at the line of junction between the sac and the ovary numerous syntial cells are seen. The tissue at this point is very vascular. In at least two points in the wall of the sac, follicles are seen. From a careful study of this case it seems safe to assume that the pregnancy has taken place in a follicle. This case fulfills all of Spiegelberg's criteria.

This specimen is from the service of Dr. John G. Clark, he having operated on the case November 11, 1907.

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THE SURGICAL TREATMENT OF CANCER OF THE SIGMOID FLEXURE AND RECTUM

With Especial Reference to the Principles to be Observed

By B. G. A. MOYNIHAN, M. D., F. R. C. S., Leeds

IT cannot truthfully be said that the surgical treatment of carcinoma, occurring in the upper part of the rectum or in the sigmoid flexure, is at the present time entirely satisfactory. Two reproaches may justly be brought against it: the first, that removal is attended, in no small proportion of cases, either by regional or by distant recurrence; the second, that colostomy has to be performed, on account of the sacrifice of a large portion of the bowel, in perhaps a majority of cases. The position at the moment seems to be this, that if only so much of the bowel is removed as will permit of end-to-end anastomosis, an adequate excision of the parts, intestine, lymph vessels, glands, and the tissues which bear them, can hardly be made; whereas, on the other hand, if these parts are freely removed, the performance of colostomy is compulsory. It is the purpose of this paper to shew that a free removal of the area involved is possible, and that with certainty in most cases, and with probability in all, an end-to-end approximation of the bowel is easily possible.

It is the first essential in all operations concerned with malignant disease that the removal of the parts shall be free, and that it shall follow certain lines. In the large intestine the growth itself must be removed, a length of bowel on each side of the growth, the lymph vessels which drain the bowel, the glands in which those vessels end (all the primary glands, that is, and as many of the secondary as are accessible), and finally all the tissue in which those glands and vessels lie. In the case of the sigmoid flexure, and of the upper part of the rectum, this will involve a removal of the growth and of healthy intestine on each side of it, and the excision of all the glands which lie along the arteries as far up as the inferior mesenteric artery at its origin from the aorta. At the point where this vessel arises a lymphatic gland is always to be found; it lies along the artery before the origin of the left colic branch, and is the highest of the chain which, beginning at the intestine (in any part), extends upwards along the sigmoid superior hæmorrhoidal arteries to the inferior mesenteric trunk. Beyond this gland, the chain is continued into the glands which lie along the aorta. This gland, therefore, which lies on the inferior mesenteric, close to its origin, must be removed, and all glands which lie below it, if the necessary conditions just enumerated are to be fulfilled. In the removal the inferior mesenteric artery may have to be ligatured, either immediately beyond its origin, or after the left colic artery has been given off. Probably in many cases, the gland can be stripped down from the vessel by firm wiping with gauze, after the peritoneum above it and on each side has been lightly divided. In two of my recent cases I have, however, found this impossible, and the artery at its origin has therefore been ligatured. This, for reasons presently to be disclosed, is not of the importance it might seem. The ligature on the artery is the summit of a wedge of material to be removed, the base of which lies at the intestine. The length of intestine which has to be excised is not a point to be specially considered, for though it may seem paradoxical it is nevertheless true that the more freely the gut is sacrificed the less likely is neurosis to follow, and the more certain is the end-to-end anastomosis to be successfully accomplished. In one of my cases, 14½ inches of the intestine were removed and end-to-end suture easily performed.

The two points of chief significance in the operation as I wish to describe it are:

1. *The mobilising and displacement of the intestine.* This is carried out by making an incision through the peritoneum at the outer side of the mesosigmoid, at the points where this mesentery springs from the parietal peritoneum. The sigmoid together with its mes-

entery is then stripped up from the iliac fossa towards the aorta, the peritoneum on the inner side of the sigmoid being lifted off the posterior surface of the abdomen until the middle line is reached. This stripping extends well upwards and downwards, until the whole flexure and the upper part of the rectum are attached only by a leaf of peritoneum on the inner aspect. At a later stage of the operation, the descending colon and the splenic flexure are similarly mobilised, by incising the peritoneum to their outer side and above the flexure and by stripping the gut inwards to the middle line. The middle and left colic arteries are in the peritoneal fold, which now forms, as it were, a mesentry for the bowel. The result of this freedom of the intestine is, that it can be quite readily drawn down so that the descending colon reaches well into the pelvis, and could, if there were need, be made to extend to the anus. In carrying out this manoeuvre the transverse colon also may be loosened, so that its central V-shaped dip is straightened. The splenic flexure is normally of course fixed high up on the left side; in its altered position it is made to descend several inches. This procedure has to be performed before it can be realised how perfectly simple it makes this displacement or transplantation (if one may so term it) of the intestine. The result of it is, that the bowel is rendered so free that it can readily be placed in such a position that the end of the descending colon can be brought into easy apposition with the divided rectum and union then secured by suture; and the vascular supply of the parts is secured by the preservation of the vessels in the peritoneal fold by which alone the mobilized gut now remains attached.

2. *The condition of the vascular supply.* It might perhaps be supposed that the severance of the inferior mesenteric artery would deprive a large part of the sigmoid and of the descending colon of its blood supply. This, however, is not the case. If the middle and left colic arteries be examined, it will be found that they anastomose at about two inches from the intestine, in a vessel which runs parallel with the bowel; from this artery straight branches pass to the intestine. That the circulation through this vessel is carried on quite freely after section of the inferior mesenteric may readily be demonstrated in any operation by loosening the clamp applied at the upper severed end of the sigmoid flexure; free hæmorrhage occurs at once. An examination of the vascular supply of the transverse and descending colons and the sigmoid shews clearly enough that the sacrifice of the direct supply to the left colic artery, by ligature of the inferior mesenteric, is not in the least likely to interfere with the easy transmission of blood through the vascular arch which the left colic makes with the middle colic above and with the sigmoid and superior mesenteric below. The important outcome of this is, that the upper divided end of the sigmoid flexure, at whatever high point the division is made, is freely supplied with blood, even after the inferior mesenteric trunk has been divided.

These two points, then, make it clear that a great length of the bowel may be sacrificed and the normally fixed parts of the large intestine above the division be rendered so mobile that their transplantation is a matter of no difficulty; and further, that the high division of the inferior mesenteric artery, made necessary by reason of its close relation to glands which it is imperative to remove, does not devascularise the upper end of the bowel which is to be engaged in an end-to-end anastomosis.

The precise details of the operation to be practised will depend upon the exact position the growth may occupy. I will first describe the procedure necessary in a case of growth about the middle of the sigmoid flexure.

The patient is placed in the Trendelenburg position and a long incision made in the middle line. A Doyen's valve-retractor now, or a little later in the operation, gives a good exposure of the pelvis. The intestines are packed away with swabs. only the rectum, sigmoid, and a part of the descending colon being at first visible. The sigmoid is lifted up from the posterior abdominal wall, and with the scalpel or scissors an incision is made in the peritoneum of the iliac fossa, immediately to the outer side of the mesosigmoid. This incision is continued down into the true pelvis, keeping close to the intestine, and up along the outer side of the descending colon. A piece of gauze wrapped round the fingers now strips up the mesosigmoid from the iliac fossa, and the

separation is continued steadily towards the middle line. The ureter is to be displayed in all the length of the incision, so that its security is in no doubt. The spermatic vessels are also seen, and are carefully handled, to avoid damage to the vein, which tears readily. The freeing of the peritoneum on the inner side of the sigmoid and the descending colon is continued until the aorta is reached, and the inferior mesenteric trunk is recognised at its origin. The bowel is now quite freely movable and can be turned well over to the right, attached by a single leaf only of the peritoneum in which the vessels lie. This peritoneal leaf is translucent; by holding it up to the light the exact line of the vessels can be seen, and the position of many of the glands defined. The raising of the peritoneum with the bowel has been carried out in such manner as to leave the pelvic wall bare, all fat and glands and vessels are raised up with the fold of the serous membrane. Into the space made bare a large hot moist swab is packed. The inferior mesenteric artery is now surrounded by an aneurysm needle at its origin; it is tied in two places with strong catgut, and divided. From this peritoneal wound two incisions are made, the one upwards towards that point where the sigmoid flexure or descending colon is to be divided, the other downwards over the aorta and along the front of the sacrum to the rectum at the place where its division is necessary. To meet the lower end of this peritoneal incision, a continuation is made of the incision already begun on the outer side of the sigmoid. The two meet across the front of the rectum or the lower part of the sigmoid. The point of their union is determined by the position of the growth. At this stage all that is necessary to remove the wedge-shaped area to be sacrificed is the division of the intestine above and below. This is effected between clamps, after sedulous care has been taken to avoid infection. The liberated mass is then removed. It may be 10 inches long at its intestinal base, or even more. The free vascularity of the upper extremity of the bowel may be demonstrated by a slight loosening of the clamp. The approximation of the two ends of the intestine would now seem perhaps to be almost impossible, but the mobilisation of the descending colon and the splenic flexure will soon render their anastomosis easy. When, however, it seems likely that this displacement of the descending colon will be needed, it is desirable to carry it out before the intestine is severed so as to be certain of a perfectly aseptic operation field at the time. The end-to-end anastomosis is then completed in the usual way.

For a growth in the rectum the same principles apply. The highest gland in the lymphatic chain, that which lies on the inferior mesenteric artery, must be taken away. Nothing less will do. The inferior mesenteric artery must then be divided, either above or possibly beyond the origin of the left colic artery. The peritoneal incision begins to the left of the upper part of the rectum and the sigmoid, and both these are wiped upwards with gauze towards the aorta. They are rendered mobile before anything else is done. The inferior mesenteric is then divided, the peritoneum incised downwards over the front of the sacrum, the middle sacral artery ligatured, and the sacral hollow wiped clean by repeated applications of gauze, stripping little by little. When this has been done, the amount of freedom which it is necessary to give the descending colon must be ascertained, and be provided. End-to-end anastomosis, when the bowel has been severed, will not always be possible inside the abdomen, for probably only four or five inches of the lower part of the rectum remain. In such circumstances the upper end of the rectum is tied with a stout catgut ligature before division. After the removal of this growth, an assistant passes a pair of forceps into the dilated anus and seizes this tied end of the rectum which is then inverted until the ligatured end can be made to protrude beyond the anus. The upper divided end of the sigmoid is then pulled through the anus with forceps and an anastomosis made by Mannsell's method. Many of the details of this procedure are the same as those laid down by C. H. Mayo (SURGERY, GYNECOLOGY AND OBSTETRICS, 1906, iii, 240). The essential differences are, that in the method I describe the bowel above is freely mobilised so that easy end-to-end anastomosis is possible, a greater length is removed and the excision of the entire glandular group is ensured. It is probably safe to say that this last,

and I think most essential, feature of the operation has never previously been suggested or adopted as a routine procedure. In the operations generally practised (Mayo's, for example) colostomy is performed too frequently. This serious drawback to the cosmetic attributes of the operation can be avoided, strange though it may seem, by a higher division of the arteries, and by a wider removal of the intestine, provided the principle of displacement of the colon be duly observed.

For low rectal cancer I have never carried out an abdominal operation, but if adequate measures are to be taken to remove the invaded area in accordance with the principles laid down, no other course than this seems rational. Operations which merely go "wide of the disease" do not meet the necessities of the case. We have not yet sufficiently realised that the surgery of malignant disease is not the surgery of organs: it is the anatomy of the lymphatic system. The inherent futility of all purely sacral operations seems to me to be quite evident.

If it is true, as I have endeavoured to shew, that the descending colon and the upper part of the sigmoid flexure retain their vitality after the sacrifice of the inferior mesenteric artery and if the mobilisation of the colon and the splenic flexure permit a considerable displacement of these portions of the bowel, then in all cases of carcinoma of the sigmoid flexure or of the rectum, whether high or low (the proctodeum excepted), an abdominal operation seems desirable, for by this route alone can the whole lymphatic territory be extirpated. The observance of the practice described in this paper should do much to abolish the operation of colostomy as a necessary part of the radical operation for cancer; though it will, of course, always be demanded for those cases in which obstruction calls urgently for relief.

HYDROPS ARTICULORUM INTERMITTENS[1]

By WILLIAM HEALY, M. D.
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DEFINITION: A chronic affection characterized by an effusion poured out into one or rarely more joints, at regular or irregular intervals, without any ascertained external cause for the recurrences, and without any perceptible anatomic alteration as cause or result of the repeated attacks. The latter part of this definition I take directly from Schlesinger, who wisely ruled out the small percentage of reported cases under this heading which showed definite pathologic changes in the joint as the probable basis of the attack.

The rarity of this disease has been witnessed to by many authors; and up to date, there have been reported only about seventy cases. A personal experience with 750 cases of recurrent effusion into the knee-joint after injury, was recently the subject of a study by Sir William Bennett; but in reporting this large number, he does not even mention any cases showing the remarkable periodicity which is the chief point of interest in the disease under our consideration. It has been suggested that many instances of intermittent hydrops are overlooked by surgeons, but in the light of the negative experience of Bennett and of other authors, as well as some very capable surgeons whom I have personally interrogated, it seems that an affection so easily diagnosed is probably not very frequently mistaken. There are but few, and then quite incomplete, analogies to this affection in the whole realm of medicine; and the interest which even a single case may awaken, can be seen by consulting the literature where we find three extensive monographs, three German University inaugural dissertations, and one Hunterian lecture, besides many long articles written on the subject.

I am able to report the unusual number of

[1] Read before Chicago Neurological Society, November 14, 1907

three cases — one of which in particular, through the intelligence of the patient, the length of time over which I have observed it, and the correspondence of the details to the usual type, offers much that is well worth close study.

CASE 1. Keen, intelligent merchant, aged 42.

Family History. Grandfather extremely rheumatic, on crutches for 25 years. Grandmother said to have had "water on knee" for years before she died. Father died at 75; slightly rheumatic, some undiagnosed lack of control of rectal sphincter for twelve years before death. Mother died aged 33 from some infectious disease. Only sister died from diphtheria aged 5. Only brother alive and well.

Previous History. As infant small and in poor general condition. Scarlet fever at 9 years. Never seriously ill since. As youth very well; athletic, especally good jumper. No malaria. Mild attack of blennorrhœa. No lues. Never more rheumatism than slight pains in muscles occasionally. A joint affection began in the fingers in 1891; characterized by chronic swelling of the joints, at first probably some fluid in them, then thickening of tissues about the joint and of the ends of the bones, gradual distortion, no pain except on pressure. This trouble progressively worse till four or five years ago, since then stationary with moderate deformity of thumbs and fingers.

History of Knee Affection. In 1887 had a gradually increasing and persistent swelling of the right knee without much pain. Attributed by patient to jumping off moving train some days previously. Had recovered recently from attack of supposed blennorrhœa which lasted only a week, and was cured by a druggist's prescription. A surgeon was called in to see the knee, tapped it, withdrew the fluid, injected with carbolic acid and put into splint. While on crutches, other knee suddenly swelled and in turn was tapped, injected and splinted. When splints were taken off, knees were found almost immovable and patient was forced to go on crutches for two years, until joints became limber. No more swelling until 1890, when suddenly without known reason, left knee swelled; but this time it disappeared in five days. Recurrence in 11 days, and now periodicity was first noticed by patient. From this time on potassium iodid and some other drugs were given without effect. Finally quinine was tried and the series of recurrences stopped. No more swelling for a year. Then after another series of periodic attacks quinine was again taken. Then no swelling for five years. At this time the right knee was injured in running for a train, and a swelling ensued which last for four or five weeks. Six months later periodic attacks began again, lasting four or five days, recurring regularly in two weeks. Quinine. After a short series no attacks for a year. Then various attacks in short series until 1901 when quinine disagreed with his stomach. Went to Martinsville (Ind.) Springs for several weeks and while there took pills of quinine, iron and strychnia. No more attacks until 1904, when in spite of all previous remedies, attacks become again well established in series every fourteen days and lasting four or five days. After about six months of this I first saw the patient.

Character of Attacks. They begin every fourteenth day with such regularity that patient arranges his business so as to stay at home on that day and for a varying number of days following, according to rapidity of disappearance of swelling. Besides this, he has always been especially careful not to injure nor over-tax his knees for several preceding days. Generalized physical symptoms have never been observed. There is no feeling of cold in legs or feet. No feeling of heat nor redness of skin over knee. Amount of pain very slight, mostly a feeling of tension, and confined to knee; absolutely no pain, if knee not moved. However, knees "never feel quite right." Either knee may be affected, but in no regular alternation. Rarely both knees have been swollen at once. No other joints have ever been affected.

Examination. Well nourished, good color, digestive functions normal. Complete neurologic examination gave negative results. Knee-jerks equal and very lively. Deformed phalangeal joints as already described. Knees show nothing abnormal except great laxity of patellar tendons. Urinary examination negative.

Treatment. Liquor potassii arsenitis in increasing doses. Because of effect on stomach could get no higher than eight drops at a dose.

Result. One slight attack at the regular interval after starting treatment and then no more for two years. One slight attack in spring 1907. Took arsenic. September 5th attack while on vacation, had no arsenic. Another attack September 17th, then took Fowler's solution for a short time. Slight attack in November, no arsenic; and again twelve days later. Then began taking atoxyl, doses half a grain, and expected attack did not occur. Stomach again became affected; patient went to the Martinsville Springs. No attacks now for two months.

CASE 2. Mrs. X., housewife, aged 36. Family history: mother had chorea as a child; died at 28 from apoplectic stroke during confinement. Father alive and well. No definite history of rheumatism in family. Patient has two children, both of whom have been treated for chorea at our clinic.

Previous History. Scarlet fever and diphtheria when a child. Inflammatory rheumatism at 13. Much tonsillitis. Always very nervous and "hysterical." Chorea of pregnancy in 1891, ill then for three months. No malaria. No injuries.

History of Knee Affection. In 1899, the right knee became swollen, the trouble not being attributable to injury or to any other cause. After a few days, swelling disappeared. In a short period it came again; and the attacks continued at intervals for six years. Sometimes one knee and sometimes the other was affected; and once both knees were swollen at one time and patient remained in bed for a week. The attacks caused no pain, but much discomfort — it was usually not necessary to keep off the feet. No general symptoms. In some attacks the swollen knee would be several inches

larger than the other one. The swelling lasted, as a rule, three or four days. The patient, who is decidedly unobservant, has never noticed the length of the interval. All sorts of local applications and bandages were tried without success. Patient does not know what medicines were given.

Examination. Seen by me at the Chicago Policlinic, February 10, 1905. Well nourished, good color. Digestion fair. Sleep good. Neurologic examination negative with exception of fine tremor of outstretched hands and tongue. Knee shows nothing abnormal.

Subsequent History. Patient was given Ac. arsenosi $\frac{1}{10}$ grain, and sulphate of quinine, 2 grains t.i.d., and told to come back whenever she had a swelling so that we might try to find out the periodicity. On March 2nd she came back with a very slight swelling of the right knee. On March 9th she returned with a worse swelling which appeared suddenly; and now the difference in the size of the knees was considerable. The patient was not seen again until November, 1907, when she came in response to a letter. She reports that she took our prescription off and on for about two months. After seeing us she had a very few slight attacks which were hardly noticeable and not worth reporting. Has certainly not had any at all for about six months. Examination again shows nothing abnormal in knees.

CASE 3. (This case is reported through the courtesy of Dr. M. L. Harris who, knowing that I was investigating the subject of hydrops intermittens, offered me the privilege of reporting the single case which he has observed in his extensive experience. To Dr. S. J. Walker, with whom Doctor Harris saw the case, I am also indebted for some of the details.)

Mrs. X., married, aged 34. Family history quite negative, with the possible exception that her mother was always rather delicate and of a highly nervous temperament.

Previous History. Up to age of 14 was considered quite delicate. Had a number of the diseases of childhood. Since 14 has been quite healthy. Menstruation regular and without any difficulty. Has two children; no trouble during first pregnancy. During the second pregnancy had nine months of almost continuous nausea and vomiting. After confinement was troubled with a severe neuralgia of face and head for several weeks. While in bed at this time remembers having a slight stiffness in the left knee which was so slight that nothing was done about it.

History of Knee Affection. About June, 1904, without any injury or apparent cause, began having attacks of pain and swelling in the left knee which became puffy and red. At first the attacks were not severe, and believing it to be rheumatism, the patient did not pay much attention to them. Gradually they became worse until she was obliged to keep quiet during the attacks and they began to recur at regular intervals of 12 to 14 days. During the attacks the joint would have the appearance of an acute arthritis with effusion. The folds of the synovial membrane could be distinctly felt, thickened and tender. In about two days, as a rule, all symptoms disappeared as quickly as they had come, and the joint would assume a perfectly normal appearance. By palpation then no swelling, thickening or effusion could be discovered and the knee could be used as well as ever.

The patient was seen by various skillful physicians and surgeons who tried many therapeutic measures without avail. At one time a cast was applied, but failed to check the series of attacks. Doctor Harris saw the patient in a number of attacks and finally, following the suggestion of various writers on the subject, gave arsenic, at the same time placing the patient's knee at rest during the attack in a very light cast. This treatment resulted in a complete cure; and the patient now after two years, remains entirely free from further manifestations in the knee.

History and Literature. Cases were described by Perrin in 1845, Grandidier in 1851, and Veillard in 1857, which were afterwards recognized as being intermittent hydrops. However, to C. H. Moore, of London, we must give credit for first studying and presenting typical cases of the disease as such. In 1864 and again in 1867, he published articles which emphasized the peculiar characteristics and especially the periodicity. Perhaps the most important papers coming next, were those of Féré who contributed an article on the history of the disease and reported more cases, viz., five, than any other author has done. Then Benda wrote a thoroughgoing monograph in 1900, including a synopsis of each of the 57 cases reported up to that date. Also in 1903, a monograph appeared by Schlesinger which was published separately and in Nothangel's *Spezielle Path. u. Therap.* In our country Brackett and Cotton presented together in 1901 a summary study with report of two cases. To students of this subject I commend the monographs of Benda and Schlesinger. In the bibliography appended to this article I mention only the later studies not included in their lists, together with the few older important works.

A comparative study of the symptoms, the course and the effectiveness of various treatment is an easy task from the considerable number of well-reported cases, but the chief interest, of course, lies in the etiology of this unusual disorder and it is to this part of the subject that we shall give most attention.

Symptoms. There is a remarkable uniformity about the character of the attacks in different individuals. In most cases there are absolutely no signs of inflammation in the joint,

in the small minority there are slight degrees of heat and redness. The amount of swelling varies greatly in the different attacks. My Case 1 always gauges the onset from the time when pressure of the hand on the side of the knee below the patella, pushes over the fluid and causes a bulging on the opposite side. Occasionally there is no increase beyond this, but often, as in other instances, the swelling can be measured by inches. Authors generally agree that the swelling is limited to the capsule of the joint and that it is a simple, serous effusion, but in at least one case the tissues about the joint were also swollen. Pain varies greatly, as one might expect in a purely subjective phenomenon. In some patients there seems to have been no pain at all connected with the attacks — at most a feeling of discomfort. In others this discomfort is very marked and described as a feeling of tension; and in the remaining few patients there may be actual pain, slight or severe, and be either confined to the joint or occasionally extend up and down the leg. General symptoms and even prodromata have been mentioned, but evidently are distinctly rare. Among the latter malaise, chill, slight fever, and backache have been reported; while during the attack, dizziness, vomiting, and tachycardia have occurred. Polyuria may coincide with the disappearance of the swelling. Function of the joint is usually more or less disturbed — the limitation of motion varying for the most part according to the amount of effusion — although some patients are able to continue at their work.

The time relations of the disease are among the most interesting points. In the majority of cases the swelling begins without warning, except such as previous experience with periodicity may have afforded, reaches its maximum in from one to three days, and takes perhaps a little longer to subside — the attacks, then, lasting from two to eight days. The interval before another swelling occurs varies with the individual, but in most cases is curiously regular. It varies from twenty-four hours, as in one of Marsh's cases, to four weeks. Benda states that there was regularity in the length of the intervals in 42 out of the 57 cases he analyzed. This regularity has in many instances partaken of the almost uncanny predictability described in Case 1, who was subject to recurrences during one period of the disease every twelfth day and at another time every fourteenth day. In the cases which I have found reported since Schlesinger's monograph appeared, the regularity of the periods has generally been a very marked feature.

The phenomena of this disorder have been so frequently limited to the knee-joint, that some authors have called the disease intermittent hydrops of the knee; but in the rarer cases — in 13 out of Schlesinger's 57 — other joints besides the knee show the effusion. The elbow, ankle and maxillary joints, particularly, may be affected with or in alternation with the knee, and even the joints of the spine in one instance showed the disturbance.

The relation of these attacks in women to the menstrual period is generally, but not always, negative; but when there seems to be direct relationship there is great irregularity in different individuals. Attacks may come only at that period, or may be merely severer at that time, or again the slighter attacks may come during menstruation. A so-called "quiet effusion" into the knee-joint in girls is mentioned by Bennett, who has seen over a dozen cases. It begins with puberty, and passes away with the full establishment of the catamenia unless the joint is injured. Then there may be recurrences at intervals. Study of these few suggestive recurrent cases is not offered by Bennett in detail — an essential to classification in our category.

Diagnosis. About diagnosis there can be no difficulty, if a case is properly observed.

Pathology. Examination of the joint between the attacks, is in nearly every case reported to yield only negative results. As I have previously stated, if gross anatomic changes exist which could in any way account for a recurrent effusion into a joint, then the case must at once be ruled out of the category of hydrops articulorum intermittens.

Etiology. As to sex, the cases are about equally divided. Two patients under 10 years of age have been reported; the others range through all decades up to 60. The onset is generally in the young adult. Occupation: Notwithstanding Schlesinger suggested that exposure and strenuous occupation may have

a predisposing influence, it certainly seems that the great majority of cases occur in those who have been under no such conditions. Constitution: As Benda remarks, this affection may be found in both the robust and weakly. Heredity: From this standpoint very little light is thrown. In only one case has there been reported direct inheritance of the trouble, viz., the instance recorded by Blanc where a mother at 22 and her daughter at 17 became subject to the swellings. On the other hand, in many cases there has been a distinct neurotic heredity, as in Case 2, or occasionally as equally distinct arthritic antecedents in the family and the individual, as in Case 1. Relation to sexual life has its chief interest in the important fact that in at least 5 cases the attacks are said to have entirely ceased during pregnancy and part of lactation.

Whether definite infectious or other diseases play an etiologic rôle in intermittent hydrops has been much discussed. Historically, the first consideration was malaria. Moore's original case had suffered from ague some years before the onset of hydrops, but Moore is quite willing to leave the question open in the light of the fact that quinine did not relieve the attacks, which were of 18 years' standing. Recently this question of malarial origin has again been gone into by Marsh, who had the remarkable case which showed swelling regularly every evening. The blood, however, was examined by a no less competent observer than Donald Ross, who found no parasites. The attacks ceased after treatment by quinine. Altogether very few cases indeed show even the remotest connection with malaria, the periodicity showing particularly no direct correspondence. On the other hand, from the standpoint of malaria itself, Ross and Manson (cited by Marsh) each with a vast experience, say that they know nothing of joint troubles caused by malaria. Bennett, however, recently seems to prove by his experience with 18 cases that an injured joint may recurrently swell during attacks of malarial fever.

Some observers have considered gonorrhœa in a possible etiologic relationship. Albert saw the attacks of hydrops occur in a case synchronously with exacerbations of chronic gonorrhœa. In a few other cases, that disorder was present at the time of onset. In one of the older cases quoted by Benda, the attacks followed on gonorrhœal rheumatism; and also in two cases which Marsh quotes Garrod as having seen. Obviously here, also, the connection is too remote to be explanatory.

The joint fluid has been aspirated and examined bacteriologically a number of times, and always with negative results except in one instance where a typhoid-like bacillus was discovered, but no control experiments were done. However, here we must remember that modern pathologists tell us that micro-organisms in joints are usually found in the subendothelial layer and not in the fluid.

With regard to rheumatism, a survey of the literature leads one to agree unqualifiedly with Benda, who maintains that there is no evidence of intermittent hydrops being a form of that disease. The attacks have not infrequently co-existed with rheumatism of various types, as any such long-standing affection may well do; but, as Schlesinger states, a good argument against its rheumatic nature lies in the fact that during the long course of the affection the endocardium remains spared.

The part which trauma plays in intermittent hydrops is not easy to decide. Both popularly and professionally joint disease is so generally attributed to injury, that it is no wonder we often find recorded some accident received previous to the first attack. As in my Case 1, however, the etiologic connection is rarely convincing. Also as in that instance, only one of the supposedly injured knees may be swollen at first. This patient also shows correspondence to another feature of several recorded cases, inasmuch as the joint once affected by intermittent hydrops is apt to show peculiar susceptibility to slight injury, resulting either in a regular series of attacks or in a more persistent swelling. This whole question has also a strong negative side, when we consider that most of the cases recorded present no history of a definite injury causing the first attack — while such causation for each of the succeeding attacks in their definite intervals is an absurd consideration. Indeed, on the other hand, in a minority of the cases the patient has been sure that there was never the slightest trauma preceding the first or any other attacks,

the latter often coming on while the patient was purposely resting.

Authors who have studied the collected material are all persuaded of the kinship of intermittent hydrops to nervous disease — although the forms of the latter, as manifested in the patient or his progenitors, may have taken widely diversified forms. A considerable number of the subjects are reported as severe sufferers from neurasthenia or hysteria. Féré reports three of the latter and one of early dementia paralytica. One recorded case had migraine, two epilepsy, three Graves disease — one of the last curiously alternating the appearance of hydrops with disappearance of the major disease. Senator's case suffered from traumatic neurosis. Another patient was subject to periodic vomiting and periodic swelling of one hip, and still another to periodic diarrhœa. One patient was a hypochondriac; another, a morphinomaniac, had the joint swellings when abstinence from morphine was enforced. The tachycardia reported as occurring in attacks is also held to be a nervous phenomenon. Intermittent hydrops shows a peculiar amenability to psychic influence; Féré and Breslauer had each a case which demonstrated the joint effusion after mental excitement. Schuchard's case missed an attack when an operation was planned. Benda's case was certainly healed and several others were modified by suggestive methods.

To one who regards with favor the theory that arthritis deformans is at base a nervous affection, the appearance during this rather rare disease of intermittent hydrops is significant — it occurred in certainly three out of the seventy cases, while four more had some chronic joint changes.

The closest analogy to intermittent hydrops in the whole realm of medicine is found in Quincke's acute circumscribed cutaneous œdema, and Quincke himself recorded a case with the two affections in one patient. Benda found reported one case of urticaria, and five cases in which there was swelling of the skin about the knee simultaneously with the hydrops. Olser and Joseph Collins have both been careful students of Quincke's disease. They call attention to the fact that it is often found in neuropaths and sometimes with Graves disease. Frequently it has been noted that occurrence of the swelling in one spot seems to predispose the place to future attacks. Both agree that periodicity may be a remarkable feature of the skin swelling. Osler cites the case of Matas in which the swelling came on every day about noon, and one of his own in whom there was intense swelling recurring every three or four weeks. However, from our present standpoint the most interesting case of Quincke's disease which I have found is recorded by Jamieson in 1883. He described a female patient of previous good health who at 53 began to suffer from arthritis deformans which progressed until there was distortion of many joints. After six years this became quiescent. During all this period and continuing afterward she was subject to sudden severe subcutaneous swellings, particularly prone to attack her eyelids, quite closing the eye, or her lips. At first these attacks recurred at intervals of about four weeks, then after some years they came once a week, particularly on Sundays. The swelling disappeared inside of three days, and its presence caused only a feeling of tightness. He found treatment with blisters and the administration of arsenic most beneficial. Notwithstanding the obvious differences surely all this likeness between the two diseases makes out a strong case for recognition of them both as vasomotor neuroses.

Pathogenesis. The fact that the knee has the most complicated and by far the largest synovial surface, explains its peculiar liability among the joints to any secretory disorders.

An instructive study of the whole question of the origin of effusion into joints is lately presented by Hildebrand. He describes the tremendous network of blood vessels supplying the capsule and synovial membrane, particularly of the knee-joint, and emphasizes the fact which several anatomists have corroborated, viz., that the blood-vessels, even the smallest arterial branches, are extraordinarily well supplied with medullated nerve fibres which enter the muscularis, and that all the smallest transitional vessels and capillaries are supplied with non-medullated fibres. Thus there exists, under the influence of the nervous

system, a thoroughly distributed and delicate mechanism capable of readily altering or restoring the normal secretory conditions. Hildebrand also tells of the peculiarly rich nerve supply to the serous surface itself. The nerve endings are there largely composed of special sense-bulbs. He suggests that through this mechanism irritations which might neither be felt as such nor cause gross anatomic alteration could readily by reflex action produce a transudate. Bennett corroborates this from the clinical standpoint in telling of his 56 cases of non- traumatic effusions following old injuries. He opened 12 of these joints, and among them found five with no internal derangement whatever. He classifies such as abnormal joints which respond with effusion to very slight repeated injuries which are not noticed. This type of case may, if Bennett's ideas are right, claim some which are now called hydrops intermittens; but such instances as Case 1, with effusion recurring while the patient is purposely resting are beyond his classification.

In a study of the etiologic rôle of the vasomotor centers in the production of angioneuroses, etc., Polland states his belief that the many cases of circumscribed cutaneous œdema, which are reported occurring with tachycardia, prove an abnormal excitability of the vasomotor center in the medulla. Perhaps the occasional concurrence of tachycardia with hyrdops might lend his hypothesis some value for us.

How to account for the periodicity. Well, Benda meets this in Yankee fashion by asking another question: How does one account for the whole list of biologic and pathologic periodic phenomena — for periodic psychoses, periodic epilepsy, for menstruation and periodic migraine?

Barker's suggestion that perhaps the effusion may be connected with lowered coagulation-time of the blood seems rather out of the way in light of the periodicity and selectiveness of location of the swelling. At any rate, these periods would greatly complicate examination of coagulation-time which is on none too sound a clinical basis now. It used to be thought that potassium iodid increased the coagulability of the blood. I do not know that this has been proven by modern research, but my observation, whatever its worth, is that Case 1 developed a new series of attacks while under treatment with considerable doses of potassium iodid.

Prognosis. Moore's original case was lost sight of after 18 years subjection to the attacks. Kamp very recently records his patient who for years has absolutely resisted all treatment. Against these and the many other pessimistic records we can balance the reports of a few brilliant cures and some other instances of marked improvement. The cases seen by American authors have, for the most part, been especially tractable to treatment.

Treatment. Nearly everything that can be imagined in the way of external local treatment has been tried — bandaging, fixation, all sorts of medicated applications, ferrum candens, blisters,— and pretty much to the same purpose, most of them having no effect at all, while occasionally some one measure seems to interrupt the cycle of attacks. But we must estimate even this modicum of success by the fact that some cases have been found liable to spontaneous betterment. Then, also, the experience of Case 1, in having the trouble banished from one knee only to appear in the opposite joint has several times been recorded, especially after operative procedures.

Internal treatment of the joint by aspiration and injection or by free opening and washing has rarely yielded good results and, therefore, is certainly contraindicated except as a last resort. In the light of published facts one is surprised that surgeons care to recommend so radical an undertaking. If there are no anatomic alterations to be found, as surgeons have maintained, then very likely any possible good results from operation are merely achieved by suggestion.

Psychic conditions, however, have been shown to be powerful factors in inducing attacks, in aborting them and even in curing the disease, therefore any legitimate suggestive treatment is certainly in order for amenable patients.

Many internal remedies have been exhibited and mostly without avail. However, there are two drugs, quinine and arsenic, which have shown some reliability. The use of the former

rests partly upon a theoretical basis inasmuch as it acts as a vasoconstrictor on the vessels of the joints. It is difficult to exactly determine from the literature the statistical value of these remedies, but evidently quinine may be credited with three cures and two satisfactory improvements, while arsenic apparently cured six and improved one. The latter proved beneficial in some cases which resisted the effects of quinine. Both drugs and especially the arsenic are best given in doses which prove moderate for the individual and over a prolonged period.

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SYNCYTIOMA OR CHORIOEPITHELIOMA OF THE PLACENTA

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THE following case presents symptoms of clinical interest, and suggests for discusion several points of practical importance:

Mrs. T. M., primipara, was admitted to the Maternity Department of the Jefferson Hospital, December 14, 1907. Her family history was negative, with no history of cardiac, renal, tubercular, or malignant disease.

She had measles and pertussis in childhood; menstruation was irregularly established, and continued irregular at intervals of from four to eight weeks. Very little pain accompanied it.

During pregnancy she had nausea and vomiting for the first two months, and afterward was fairly well. When she first came under observation the uterus was three fingers' breadth above the umbilicus, heart sounds were heard in the left lower quadrant, the placental sound was not heard, the fœtus was in first position, vertex presentation, and the pelvic measurements were normal. The urine contained a thin cloud of albumin, but was otherwise normal.

The patient was delivered of a still-born child on December 14, 1907. There was very slight laceration, which was immediately repaired. The labor was unusual from the occurrence of hemorrhage, which preceded and accompanied the birth of the child. There was no placenta prævia, and the source of hemorrhage seemed to be a premature placental separation. The child was still-born, weighing 3 pounds, 14½ ounces.

According to the patient's history, she was at least nine months advanced. The child had no apparent abnormalities and was not macerated. Its length was 41 cm., its proportions normal for its length and weight. On making efforts to establish respiration with forced expiration, bloody serum came from the mouth and nose. An autopsy could not be obtained.

The placenta weighed 11½ ounces, was intact, the length of the cord, 42 cm.; the cord was laterally inserted, bloodless and shrunken. The placenta was evidently abnormal, containing several areas of grayish-pink sponge-like substance. It was sent to the Pathological Department for minute examination. The report of the Laboratory is as follows:

The specimen is a placenta, measuring 16.5 by 10 by 2.5 cm., and weighing 267 gms.

The umbilical cord, attached at the periphery, is 35 cm. in length and .5 cm. in thickness.

On the fœtal aspect of the specimen the vessels are large and tortuous, and the surface is covered by a thin, transparent, smooth and glistening membrane beneath which is a corrugated, reddened surface.

The maternal side is for the most part dark red in color; in some areas it is rather firm, circumscribed, and slightly elevated, appears spherical, is greyish-pink, and varies from 1 cm. to 2 cm. in diameter. These

places cut with little resistance; the incised surface is greyish-pink, very granular and sponge-like. The areas just described, extend into the underlying substance for a distance of .5 cm.; some appear to possess a capsule which is less than 1 mm. thick, fairly adherent and pinkish red.

The placental substance is rather soft and flabby, and cuts with ease; the incised surfaces are dark red, spongy, rather moist and ooze blood. On the fœtal membrane .5 cm. from the margin, is an isolated mass of placental tissue, 4.5 cm. in diameter. It presents nothing different from other parts of the placenta; 3 cm. from one margin of this mass and 1 cm. from the placental margins is a nodule in the fœtal membrane 1.9 x 2.2 x 0.5 cm.; it is firm, resistant, evidently between the chorion and amnion, and cuts with little resistance; the incised surfaces are greyish-white, spongy and very coarsely granular. The amnion is smooth and glistening. The chorion is rough and contains fragments of reddish-pink tissue.

Cubes were fixed in Heidenhain's solution, embedded in paraffin, and stained with hematoxylin and Van Gieson.

Histology. The conspicuous feature of these sections is the chorion; it is not limited to the margins of all the sections but penetrates the underlying structures as wide bands, and at some points is found in large circular masses. It is composed of many cells and an intercellular substance. The elements are large, as a rule polyhedral, and sometimes oval. The nuclei are ovoidal, central, poorly staining, transparent and reticulated; the meshes contain a few chromatin granules. The protoplasm is abundant and rather feebly staining and very granular. These cells are enclosed by the intercellular substance. At no place does no alveolus contain more than five or six cells, frequently but one. The arrangement is much like the cells in cancer. The intercellular substance is not fibrous tissue; its morphology and tinctorial properties resemble keratin, although it is somewhat granular. The chorionic tissue at some points shows extensive necrosis, disintegration, and among the fragments of tissue there is considerable blood. In the necrotic tissue the outlines of the alveoli are clear, but their contents cannot be identified. In a few sections, the chorionic tissue is limited to the margins. The villi vary in size; in structure they are similar to those in normal placenta except that in some the layer of Langhans, the syncytial strata and the cell balls are absent. In others the syncytium can be identified, and in still others only the cell balls are seen. The vessels of the villi vary in size, but all are filled with fairly well preserved erythrocytes. The margins of the villi are well bathed with blood.

Diagnosis. Chorioepithelioma or placentoma.

The patient's puerperal period was not complicated by infection, nor did hemorrhage recur. She was evidently anæmic, and the blood count showed hæmoglobin 55 per cent, red cells 4,140,000, leucocytes 10,200. Under treatment addressed to the correction of this anæmia, the hemoglobin increased to 64 per cent, the red cells to 5,140,000, the leucocytes diminished to 5,840.

Vaginal examination at the close of the puerperal period showed the uterus fairly well involuted, the cervix very slightly lacerated, the pelvic floor and perineum sound, and a very slight dark bloody discharge from the uterus.

In view of the pathological findings in the placenta, dilatation and curetting were performed under ether one month after the birth of the child. The uterus was more soft than normal, dilating readily, and abundant spongy endometrium was brought away by the curette. The scrapings were carefully preserved and sent to the laboratory. The patient made an uninterrupted recovery from the curetting, involution became complete, and the patient's general health steadily improved. No abnormality in the pelvic organs could be discovered when the patient was examined under ether.

The laboratory examination of the scrapings showed normal endometrium with no evidence of malignant disease.

Upon receipt of the result of the examination of the placenta, it was first determined to subject the patient to hysterectomy, and she gave her consent to this operation. It seemed, however, possibly not justifiable to sacrifice the uterus because of the diseased condition of the placenta, especially as I could find in the literature of the subject no precedent for so doing. Accordingly, it was determined to curette the uterus instead, and if the scrapings proved malignant, then to perform the hysterectomy. As the scrapings showed no sign of malignant disease, the patient was discharged.

In addition to this case, the writer has reported the case of a multipara pregnant a few months, suffering from pernicious nausea. Other treatment failing, the uterus was emptied under ether. The patient grew worse, complaining of severe headache, and died a few weeks after operation. Autopsy showed chorioepithelioma in the important viscera of the body, the pelvic organs remaining normal.

In a third case, the patient died after abortion from persistent oozing hemorrhage, which could not be controlled. Packing with adrenalin solution temporarily stopped

the bleeding, which returned when the packing was removed. The uterus was slightly enlarged, soft, and dark red. There was, however, no distinct evidence of malignant disease, and no autopsy could be obtained.

So far as could be judged, the patient's death resulted from cardiac syncope. There were no definite symptoms, and in the absence of an autopsy it could only be surmised that the case was one of chorioepithelioma.

Examination of the blood and urine were negative, with the exception of demonstrating a profound anæmia.

The literature of the subject is so extensive that we quote only a few of the most recent papers.

Eden reports a case of chorioepithelioma of the vaginal wall and of the body of the uterus treated by radical operation. Death followed, and autopsy showed abundant metastases. There was no clear history of pregnancy or recent abortion, except that of repeated hemorrhage.

In discussing the theory of the growth, it is thought to be of teratomatous origin, possible in both male and female, and in view of this fact, not necessarily connected with pregnancy. Pick's case in a girl 8½ years old; Landau's 5 cases in girls from 16 to 24 years of age; Schlagenhaufer's 2 cases of chorioepithelioma of the testicle in the male subject; Emanuel's collection of 14 cases show that the tumor may occur in both sexes independent of pregnancy.

Devitzky reports from the clinic at Moscow, a typical case in a woman, aged 75 years, who had never been pregnant. This, and the preceding cases show the independence of the disease as regards pregnancy.

Schmauch (SURGERY, GYNECOLOGY AND OBSTETRICS, September, 1907) gives an extensive review of the literature of the subject in which I find no mention of disease of the placenta. He believes that there is only a gradual difference between the simple invasion of the uterine muscles by villi and their sequelæ, such as adherent placenta, placental polyps and moles, between the localized chorioepithelial growths, limited to the uterus and placental region, and villous infarcts developed by deportation of villi; and finally, between those cases in which the lungs were able to dispose of scattered fœtal cells and the typical chorioepithelioma malignum which becomes generalized by metastases. He believes that the term "malignant chorioepithelioma" should be limited to localized growths of chorioepithelial character, villous infarcts, and the typical tumor.

He summarizes briefly 206 cases, in a few of which the fate of the patient is not specified. With very few exceptions, the result was uniformly fatal for the mother. There is no mention of the fate of the infant in cases occurring after viability.

An analysis of this and other cases shows that in most patients the disease manifests itself in the first four months of pregnancy.

Sternberg reports an interesting case of chorioepithelioma of the cervix in which decidual cells from the uterine decidua could be demonstrated.

Swayne reports two cases, one of which recovered apparently after vaginal hysterectomy, and the other died with metastases in both lungs. Both followed early pregnancy.

Stone summarizes 22 papers upon the subject, quoting Teacher's collection of 188 cases, of whom only 19 were at full term, with a mortality rate of 63.6 per cent. He reports the case of a young woman with irregular menstruation, examined by a physician who, finding a persistent flow, advised curetting. The scrapings were diagnosticated as sarcoma, and an abdominal hysterectomy was performed. The patient recovered.

The operator who performed the hysterectomy was not informed concerning the history of the case, and on studying the case more closely, recognized his mistake. Re-examination of the slides showed that chorioepithelioma, following abortion, had been present. But two spontaneous cures were authentically reported; while the mortality among the cases operated upon by hysterectomy is estimated at from 59 to 63 per cent.

That chorioepithelioma of the uterus may coexist with lutein cysts in the ovaries is demonstrated by Blacker in a case operated upon at the University College Hospital.

Hicks collected 14 cases of primary vaginal growth, and reports one himself. The

pregnancy had advanced to five and a half months. The patient died from metastases.

He appends a table of the 14 cases of vaginal growth which he has collected.

That chorioepithelioma may be accompanied by hæmatometra is illustrated by a case reported by Griffith and Williamson. Here coagulated blood occluded the cervix, hemorrhage continued, the uterus becoming greatly distended. The ovaries contained lutein cysts.

Garkisch reports chorioepithelioma between the layers of the broad ligament upon the left side. Although the uterus was removed with its appendages, the patient died with pulmonary metastases. The uterus was not involved, and the origin of the growth was thought to be the isthmus of the Fallopian tube.

The coincidence of lutein cells in both ovaries, with chorioepithelioma of the uterus, is further emphasized by Blacker. He reports the case of a multipara who had chorioepithelioma of the uterus and vagina. After operation the patient suffered from hemorrhage, with rise of temperature to 102 degrees F., and rigors. She had cough and expectoration of blood, but finally recovered.

Vineberg reports two cases, the first having the characteristic uterine growth, and the patient recovered after hysterectomy.

The second case was treated by curetting, making at the time a very good recovery. The uterus, however, continued to increase in size, and hysterectomy was successfully performed.

Klinge reports a case which recovered after the extirpation of the uterus by the cautery. The patient had the characteristic uterine growth, with hemorrhage and typical symptoms.

A most interesting review of the pathology of the condition is given by Risel. The bibliography of 264 papers is reviewed, and attention is called to the doubt which has existed as to whether the tumor was maternal or fœtal in origin.

The relation between pregnancy, abortion, the occurrence of mole, and ectopic gestation, is tabulated from the analysis of the papers of Ladinski, Teacher, and Briquel, aggregating 533 cases. Of these, between 36 and 41 per cent followed molar pregnancy; from 22 to 28 per cent normal gestation; from 31 to 33 per cent abortion; and from 2.3 per cent to 3.7 per cent ectopic gestation.

While attention is called to epithelial proliferation and to the growth of syncytial tissue, no distinct mention is made of the recognition of these masses in the placenta itself.

Kroemer's paper contains an interesting statement concerning the malignancy of the growth. In the clinic at Geissen, during seven years, 3856 cases of pregnancy were under observation, among whom 3,841 had normal development of the ovum, 15 had vesicular moles, and 299 had abortion. From those in which the embryo developed normally as far as pregnancy went, there were 2 fatal cases of metastatic chorioepithelioma. Among the vesicular molar pregnancies, numbering but 15, there were 5 cases of chorioepithelioma, all of whom recovered after one or several curettings. When all the cases of pregnancy were considered, the mortality from this cause is estimated at .18 per cent: when the embryo developed normally, at .05 per cent; and when molar pregnancy developed, at 33⅓ per cent.

We desire to raise for discussion three points: 1. The importance of examining, in all cases where pregnancy is interrupted after the fourth month, the placenta as far as possible. While we have failed to find distinct mention of placental involvement in this disease, we cannot believe from our knowledge of its genesis and growth that the placenta is not involved.

Ballantyne's statement that the placenta is the key to fœtal pathology should be kept in mind in all cases where the fœtus is still-born, or where pregnancy is interrupted after the placenta is formed. Theoretically, this growth should begin in the placenta from chorionic villi, and the examination of the placenta, and the finding of this growth would indicate that the patient had resisted the disease during the early period of gestation and that the growth had possibly become localized in the placental tissue. Should the placenta be abnormal and found to contain chorioepithelioma, the attention of the obstetrician must be immediately directed to the study of the mother.

All cases of pregnancy complicated by anomalous hemorrhage must arouse the suspicion of chorioepithelioma malignum. In

the first case reported, the one abnormality in labor was hemorrhage, which suggested placenta prævia, or accidental separation of the placenta. The latter was indeed present, but the cause of the separation was chorioepithelioma.

If a diagnosis be made of chorioepithelioma from examination of the placenta, what is the duty of the obstetrcian as regards the future health of the mother? While it is perfectly possible, as Kroemer has shown by a series of between 3,000 and 4,000 cases of pregnancy, with but 7 cases of chorioepithelioma, 2 of which proved fatal, for spontaneous recovery to occur, one must remember the comparative mortality of those cases allowed to go without operation, and the mortality of those subjected to hysterectomy and other operations. The literature of the subject shows but 2 accurately reported cases of spontaneous recovery — a very large mortality.

In 96 cases subjected to the operation of hysterectomy, a mortality of 63.6 per cent followed — the lowest mortality estimated as following operation being 59 per cent. The cases subjected to operation, however, have been those in which a definite change in the uterus or its contents, as shown by curetting, could be demonstrated. We must remember that fatal cases occur in which the uterus is not involved.

Shall the operator who detects chorioepithelioma of the placenta proceed immediately to hysterectomy as soon as the patient's condition justifies it, or shall he content himself with a thorough examination of the patient under ether, including curetting, and the microscopic examination of the scrapings?

If these procedures give no evidence of chorioepithelioma, is he justified in allowing the patient to pass from observation without hysterectomy?

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IV. THE DAWN OF MODERN MILITARY SURGERY

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THE dawn of modern military surgery was foreshadowed by the epoch-making teachings and example of men like Hilden, Baron Larrey, and others, who took an active part in the fierce wars of the latter part of the eighteenth and the beginning of the nineteenth century. The blind faith in ancient authorities and the handmaid of ignorance, superstition, gradually vanished before the onset of reason and critical thought. During this time, the character of gunshot wounds remained about the same, as few improvements were made in firearms and projectiles. While military surgery became more and more rational, the results of improved methods of wound treatment, a corresponding betterment of wound healing was not realized. Suppuration, pyemia, erysipelas, gangrene, secondary hemorrhage, hospital gangrene, claimed their victims, as heretofore, under less skillful treatment.

Let us now inquire into the methods of treat-

ment of gunshot wounds during the Crimean War, in the beginning of the second half of the nineteenth century. It was one of the most desperate and cruel wars the world ever witnessed. It has been my good fortune to have seen the endless earthworks around the city of Sebastopol, the vast barren plain of Inkermann, the little city of Balaclava, and the very spot where the famous eight hundred met their fate, with cannons spitting fire and destruction in front of them, and from the low bluffs to the right and to the left of them; and the acres of cemeteries where thousands of heroes have found their final resting-place. It was during this war that climate, deathly epidemics, and the most dreadful forms of wound infection combined to decimate the ranks of the besieged and the besiegers, and made up a picture of misery and suffering almost unparalleled in the history of civilized warfare. Nicholas Pirogoff, the distinguished Russian surgeon, served fourteen months in and around Sebastopol, and witnessed all the horrors of purulent edema, pyemia, hospital gangrene, and erysipelas.[1] The prevalence of these, the gravest forms of wound infections, and their spread from patient to patient, from hospital to hospital, and from place to place, caused Pirogoff to assume the existence of a miasm which he believed could be carried in the clothing and dressings and on the hands and instruments. He enforced segregation of the infected and their attendants, and insisted on thorough ventilation of the hospitals. Instruments, charpie, and other dressing materials were likewise kept from being exposed to the miasm. He forbade surgeons to explore gunshot wounds with the finger. All of the dressings were burned after use. That these precautions against infection, established on a rational basis, contributed much in lessening the frequency of wound complications we can readily conceive; but we know why they did not suffice in eradicating them. He condemned the use of hair mattresses in war, as he considered them carriers of disease. He insisted that the infected cases should not return to their quarters so soon after recovery, for fear that they might carry the contagion with them. He considered the contagiousness of wound infection as established, but admitted that the period of incubation had not been determined. He has seen erysipelas and pyemia as endemic diseases affecting isolated parts of an army, while others were free from these diseases. He defines war, from a medical standpoint, as "a traumatic epidemic." His great experience as a military surgeon, his accurate observations, and his logical reasoning brought him to the conclusion that the war department, and those who are directly concerned in the management of a campaign, are directly responsible for many of the diseases which afflict armies, a claim which modern experiences in warfare have well established. Pirogoff made a careful and extensive study of the deformation of bullets, and gives a lengthy description of their varieties and causes.

George H. B. Macleod, who participated in the Crimean War on the English side, made some very interesting and valuable observations on gunshot wounds.[2] He opens the subject with these significant sentences: "The new philosophy has dispelled the mystery, but left us still to study the eccentricities which so often mark these injuries. The contused appearance and unavoidable sloughing of the walls of the ball's track, the little suspected but serious destruction of deep parts, and the grave consequences which may ensue from such a wound, appear to have been the circumstances that suggested the envenomed nature of gunpowder, and the cautery-like action of its projected balls, as well as the idea which prevailed that, in order to get quit of the injurious influences thus exerted on the wound, it was necessary to pour into it burning oil, or curious mixtures concocted from the most opposite and absurd ingredients, or to smear the part with nauseous grease and "charmed salves."

In describing the appearance of the wounds of entrance and exit, he says, if the speed of the bullet is great, and no bone has been struck, then they are nearly alike; but if the flight of the bullet is so far spent as to be retarded by contact with the body, especially if it has encountered a bone or a strong aponeurosis, so that the speed is considerably diminished

[1] Grundzüge der Allgemeinen Kriegs Chirurgie, 1864.

[2] Notes on the Surgery of the War in Crimea, with Remarks on the Treatment of Gunshot Wounds, 1858.

before it passes out of the body, then the wound of exit will considerably exceed in size the wound of entrance. This is especially true of conical bullets. He attributed the frequency of suppuration of gunshot wounds to muscle contusion, but admits that, in exceptional cases, healing in such instances can take place by primary intention. Conical balls are more destructive to bone than round bullets, causing a greater amount of comminution. Nerves and arteries are more likely to escape from the ball than veins. In speaking of the freaks of bullet deflections, he says: "I have known it to enter above the elbow and be removed from the opposite axilla."

Foreign bodies in wounds are very common and of the most heterogeneous kind. As the result of his experience, he became partial to the precept of Begin, who argued in favor of early extraction of the bullet. In support of this practice, he alludes to the observations of Hutin, the chief surgeon of the Hôtel des Invalides of France, who saw 4,000 patients who had a ball lodged in the body, and of these only 12 were without complaint, and in 200 of these the wounds opened and closed until the foreign body was extracted. Foreign bodies carried by the ball into the tissues often do the most harm. He is full of praise for chloroform anesthesia as a valuable aid in the examination of gunshot wounds and removal of bullets.

The following method of primary dressing of gunshot wounds was employed by the English surgeons: "The stuffing in of great tents was, I need not say, unknown; and though we ascribed wondrous virtue to cold water, it was not on account of any 'magical or unchristian' power which we supposed it to possess. Water dressings, and the lightest possible bandaging consistent with the fulfilment of well-understood ends, were prevalent in our army, but not to the same extent among our allies, who have not yet given up the weighty pledgets of charpie and much fine linen, which so greatly astonish our English surgeons."

During the Crimean War, light cold-water dressings were employed on both sides as the principal covering for wounds. Oil, wine, spirituous lotions, salves, and plasters, formerly so constantly in use, were forsaken for the simplest kind of local treatment. Pirogoff protested against the use of the finger as a means of exploration of wounds, but the equally dangerous probe remained in constant use. The search for bullets and other foreign bodies and loose fragments of bone was kept up unremittingly. This meddlesome surgery, and the absence of anything like antiseptics in the treatment of wounds, account for the many grave wound complications which made the field hospitals veritable pest-houses. Air simple, and air contaminated with the effluvium of the infected in the crowded hospitals, were looked upon as the essential agents in the causation of the fatal disasters of the wounded, and led Pirogoff to argue in favor of small hospitals. In the mean time, the German military surgeons were given ample opportunity to study the effects and treatment of gunshot wounds. At the head of the progressive men of that time stood Stromeyer, who wrote extensively and most intelligently on military surgery.[1] In speaking of the improvements of firearms, their increased range and accuracy of aim, he expressed the belief that with the forging of rifled cannon further inventions would come to an end. He laid down many maxims which hold true to-day. One of these was to the effect that the harder the material of which the projectiles are made, the greater their destructive effect; and another, the difference in the character of the wounds inflicted depends solely on the caliber of the bullet. Conical bullets flatten the same way as round bullets when they strike bone. Probably the conical bullet displaces large vessels and nerves more than the round bullet. Dupuytren and Blandin claimed that the wound of exit is not always larger than the wound of entrance, an opinion which Stromeyer strongly indorsed. He says, if the bullet does not strike any hard object and passes through the soft tissues the wound of exit may be a simple slit in the skin. A bullet passing through muscle tissue carries away fibers which, when gathered, as was done by Simon, correspond with the size of the track made by the bullet. The hole made by a bullet in aponeurosis and elastic tissue is smaller than in muscles and skin. Round tendons are displaced by the bullet except when

[1] Kriegsheilkunst, L. Stromeyer, 1861, 2d ed.

they are fixed in a groove. The only tendons Stromeyer saw severed by a bullet was the long head of the biceps in the bicipital groove and other tendons similarly immobilized. He believes cerebral commotion is more likely to be produced by bullets which have lost their force than in their rapid transit, as shock is less when the skull is fractured. For the hyperesthesia following gunshot injuries of nerves, he has found the ointment recommended by Professor Schrörer, of Freiburg, very satisfactory. This ointment contains marrow of ox-bone, lard, and sweet cream. The relatively rare occurrence of hemorrhage in gunshot wounds he attributes to the elasticity of the arteries which are pushed aside by the bullet, and to the lacerated condition of wounds of arteries, as is seen to best advantage in injuries inflicted by common balls. Aneurysmal verix is prone to occur if a small bullet injures an artery and adjoining vein at the same time. The small copper bullet used by the Tscherkessen, which weighed only 120 grains, splintered bone as extensively as the large caliber lead bullet of the Russians, a fact first observed and described by Pirogoff. Stromeyer states that a bullet can penetrate one of the large long bones without splintering, and cause lateral defects without producing fracture. Cold-water dressings constituted, at that time, the orthodox local treatment; and venesection was frequently practiced, with a view of preventing or modifying subsequent inflammatory reaction. Stromeyer in all his extensive experience never saw a gunshot wound heal by primary intention. He saw wounds of soft parts heal in eight to fourteen days, but he does not call this healing by primary intention. He regarded the entrance of air and oxygen into wounds as the real cause of inflammation and putrefaction. He never observed a case of hospital gangrene among the wounded soldiers, and believes this disease has become very rare since water dressings have taken the place of charpie. He is opposed to the practice of Bilgür and Baudens, who made large incisions in gunshot fractures for the purpose of resecting the fractured ends, as he considers such a procedure very dangerous, and emphasizes the necessity of treating such fractures in the same conservative way as a simple fracture. Stromeyer was conservative in the matter of searching for bullets, and insisted that young surgeons should not be furnished with a probe, as he was opposed to meddlesome surgery, and placed great stress on rest, general and local, in the treatment of all gunshot injuries.

The Medical and Surgical History of the Civil War deals with the last great war during which the wounded were treated on the same principles as laid down by Stromeyer. Water dressings were in general use. Primary amputation or resection was the rule in all grave gunshot wounds. Suppuration was the rule; healing by primary intention the exception. Resection of any of the large joints was attended by a frightful mortality. Pyemia, erysipelas, and hospital gangrene were rampant, and claimed thousands of our brave soldiers. It was the last war during which hospital gangrene prevailed to an alarming extent. It was in the treatment of this disease that one of the strongest antiseptics, bromine, finally came into use, and yielded excellent results. Hospital gangrene was first described, as a special form of wound infection, by La Motte at the beginning of the eighteenth century; and a treatise on it was found in the posthumous writings of Poteau, in 1783. It was called hospital gangrene, because the disease prevailed mostly in military and crowded civil hospitals. In 1824, of 22 cases of amputation performed in the Hôtel Dieu, Paris, every one of the patients contracted the disease. All the large civil hospitals in Europe were invaded by the disease; and the slightest injuries and most insignificant operations often proved fatal from this complication. A very minute description of hospital gangrene is to be found in an English text-book on military surgery, which was in the hands of many of our surgeons during the Civil War.[1] When this war broke out, the Nestor of American surgery, the late Professor S. D. Gross, seized by the occasion, wrote a little work to be used as a guide by our surgeons.[2] The rules laid down in this manual represent largely the work done by our surgeons on the Federal side.

In the treatment of gunshot wounds, the

[1] Outlines of Military Surgery, by Sir George Ballingwall, 1852, 4th ed.
[2] A Manual of Military Surgery, etc., by S. D. Gross, 1862, 2d ed.

greatest stress is laid on the management of shock by the administration of stimulants, careful arrest of hemorrhage, and the prompt removal of all foreign substances from the wound, even the smallest blood clots. This was the typical treatment of recent gunshot wounds. Primary amputation was in favor, as experience in the Crimea had shown that the success of amputation was very fair when performed early, but most unfortunate when it was delayed any length of time. Flap and circular operations are recommended, and the former is given the preference in cases in which time is an important element. The flaps were sutured and supported by strips of adhesive plaster, and the stump placed on a pillow and covered with a water dressing. Bleeding was arrested by tying with waxed silk, the ends of the ligatures cut short, the long ones brought out and fastened on the surface of the flap.

"The best local applications are the water dressings, either tepid, cool, or cold, according to the temperament of the patient, the tolerance of the parts, and the season of the year." "The best plan, almost always, is to leave the opening or openings, made by the ball, free, to favor drainage and prevent pain and tension."

In speaking of hospital gangrene it is stated: "The proper remedies are sequestration of the patients, the free use of the nitric-acid lotion, iodine to the inflamed skin, charcoal, port wine, or yeast cataplasms, and frequent ablutions with disinfecting fluids, aided by opium, quinine, tincture of iron, lemon juice, and other supporting means. Mopping the affected surface freely with strong nitric acid often answers an excellent purpose. The favorite remedy of Poteau was the actual cautery." It is very strange that this terrible disease, this scourge of the military hospitals and of the larger European civil hospitals, which raged to such a fearful extent from the beginning of the eighteenth century until antiseptic and aseptic surgery came into general use, has disappeared from the face of the earth, never to return.

LANCE, SABRE, AND BAYONET WOUNDS

Among civilized nations, the lance has almost entirely disappeared as an implement of warfare. On the whole, lance wounds are not dangerous, as the majority of those injured with this weapon have recovered, even under the ancient unskillful treatment. In speaking of saber wounds, Dionis emphasizes the necessity of inspecting the weapon with which the injury was inflicted in case a part of the blade has been broken off in thrust wounds, in order to ascertain definitely the size and shape of the point lodged in the body. In the extraction of such a foreign body, he places much importance on withdrawing it in a straight line, in order not to inflict injuries on adjacent important vessels and nerves. Guthrie regards lance wounds as being less dangerous than bayonet wounds, as the latter are smaller, and consequently the escape of wound secretions more difficult. Saber wounds are clean cut, incised, or stab wounds, and unless important organs have been injured, or large blood-vessels severed, heal under the simplest non-meddlesome treatment. Much has been said recently concerning the utility of the bayonet as an arm. Some military authorities have been inclined to doubt its value as a weapon of war.

Guthrie[1] entertains a poor opinion of the bayonet as a weapon, as may be seen on page 15: "Opposing regiments, when formed in line, and charging with fixed bayonets, never meet and struggle hand to hand and foot to foot, and this for the very best possible reason, that one side turns round and runs away as soon as the other comes close enough to do mischief, doubtless thinking that discretion is the better part of valor. Small parties of men may have personal conflicts after an affair has been decided, or in the subsequent scuffle, if they cannot get out of the way fast enough. The battle of Maida is usually referred to as a remarkable instance of a bayonet fight; nevertheless, the sufferers, whether killed or wounded, French or English, suffered from bullets, not bayonets." "The siege of Sebastopol has furnished many opportunities for partial hand-to-hand bayonet contests, in which many have been killed and wounded on all sides; but I do not learn that, in any engagements which have taken place, regiments advanced against each other in line and really crossed bayonets as a body; although the individual bravery of

[1] Commentaries on Surgery, 1855.

smaller parties was frequently manifested there, as well as in the war in the Peninsula."

It is very natural to assume that the long-range firearms and the present strategy of warfare will reduce the employment of the bayonet as a weapon to a minimum; but the vicious bayonet charges of the Japanese soldiers during the late war with Russia have demonstrated that, under certain conditions, the bayonet has to be relied upon as a final weapon in storming a stronghold.

ARTILLERY WOUNDS

Artillery wounds are the same now as when the giant firearms first were employed in war. Cannon-balls, grape-shot, canister, and fragments of shells inflict torn, lacerated, and contused wounds, wounds notorious for their great receptivity to infection. The large, open, gaping wound, with a wide zone of tissue devitalized by the severity of the vulnerating missile, is only too often infected by the entrance of foreign matter from the very start, and the parts on the surface of the wound present the most favorable conditions for the growth of pathogenic micro-organisms. Some of the ancient cannons fired missiles of wood, stone, or metal of prodigious size. Ballingwall [1] relates that when Mahomet the Second besieged Constantinople, in the year 1453, he battered the walls with stone balls, and his pieces were some of them of the caliber of 1,200 pounds; but then they could not be fired more than four times a day. Other shot are mentioned as of 800 pounds weight, and one gun, stationed at the entrance of the Dardanelles, is said to carry a shot of 1,100 pounds. Stone shot of 150 pounds weight were thrown by Tippoo's army at the siege of Mangalore in 1783. If a cannon-ball strikes any part of the head, chest, or trunk, instant death is the usual result. Tangent shots, without any apparent injury of the skin, produce frightful subcutaneous injuries, which were formerly wrongly attributed to "windage," that is, violence caused by the air set in violent commotion by a cannon-ball passing near the injured person. As the result of his own extensive observations, Baron Larrey became convinced that soliders were sometimes killed instantaneously by what was known as "windage" of balls, there being no external wound, but as in such instances post-mortem examination always revealed grave internal injury, he believed there must have been a blow by the projectile, in spite of the absence of surface lesions, an opinion which is now held by all military surgeons. This observation of Baron Larrey was not a new one, for it is stated by Johnson that "when another Dutch war broke out in 1672, Sheffield went again, a volunteer, in the ship which the celebrated Lord Ossory commanded, and then made, as he relates, two curious remarks: 'I have observed two things which I dare affirm, though not generally believed. One was, that the wind of a cannon bullet, though flying, is incapable of doing the least harm; and, indeed, were it otherwise, no man above deck would escape.'" (Ballingwall). Baron Larrey furnished the post-mortem proof of the correctness of this observation. If a cannon-ball strikes a limb, it may sever it completely, or leave it dangling by shreds of skin, tendons, and aponeurosis. It was in such cases that the ancient military surgeons did not hesitate to complete the amputation, by severing the limb and sawing off the projecting end of the bone or bones on a level with what appeared to them healthy tissue.

It becomes clear that the early dawn of modern military surgery is remarkable for both what was done and what was not done. The positive part of the progress was the substitution of the ligature for the actual cautery, burning cotton and filthy hemostasis, primary amputation in all grave gunshot injuries, the gradual abandonment of meddlesome surgery in the way of frequent dressing of wounds, and the too often resort to the finger and probe in searching for bullets. The negative part consisted in keeping wounds free from obnoxious, harmful lotions, salves, plasters, and replacing them by harmless light water dressings. Superstition gave way to reason, under the teachings and leadership of such masters as I have mentioned; and in doing so, prepared the soil for the reception and germination of the seeds of modern military practice.

[1] Outlines of Military Surgery, 1852, 4th ed.

URETERAL CALCULI[1]

With a Further Report on the Combined Intra- and Extra-Peritoneal Uretero-Lithotomy

By JOHN H. GIBBON, M. D., Philadelphia, Pennsylvania.

As my prime object in choosing ureteral calculi for the subject of my paper is that of urging the advantages, especially in doubtful and difficult cases, of the combined intra- and extra-peritoneal operation, and as the demonstration of these advantages rests upon the experience had in six recent cases, probably my object would be better served and the subject better opened for discussion by a rehearsal of the histories of the different patients. Two of these cases have been previously reported in *The Annals of Surgery*, May, 1906.

Case 1. An Italian, 39 years of age, male, admitted to the Pennsylvania Hospital on August 3, 1905. The patient spoke English very badly but was sent in by his physician as a case of subsiding appendicitis. He gave a history of repeated attacks of pain in the abdomen, accompanied by vomiting and diarrhœa. The attacks lasted for three or four days, when he was able to get up and go about. Until the present attack he had been well for a year. This attack had lasted for three or four days and was accompanied by vomiting. Excepting for some slight tenderness in the right iliac fossa the patient at the time of his admission was free of symptoms. There was no tenderness over the kidney and he complained of no urinary symptoms; there continued a persistent tenderness on deep pressure in the right iliac fossa. He improved so much in a few days and the tenderness was so slight that my first inclination was to allow him to go out and return later if he had another attack. In other words, I was not prepared to accept the diagnosis of appendicitis. In view of the history, however, I concluded to operate. The patient's urine showed a number of red-blood corpuscles. Under chloride of ethyl-ether anæsthesia I operated on the 8th of August. The abdomen was opened through the right rectus; and when searching for the appendix my finger came in contact with a hard mass just below the iliac vessels in the true pelvis. It seemed about the size of a hazelnut, was very hard, and at first immovable. Although shaped like a gland, it was hard and there were no other glands enlarged. As it was in the line of the ureter I concluded it was probably a ureteral stone. I left it for further investigation and proceeded to examine the appendix. This organ showed evidences of an old inflammation, the vessels over it being very tortuous and there being numerous adhesions all about it excepting at the extremity. These adhesions bled quite freely when divided. The appendix stump was crushed with forceps and inverted by a purse string suture of gut. In order to make a more thorough examination of the small mass in the pelvis, I enlarged my incision downwards through the rectus. I was then able to determine that the mass was a stone in the ureter, and decided to remove it extra-peritoneally without, however, making another incision through the abdominal wall. The original incision was increased downwards and the peritoneum stripped from the abdominal and pelvic wall down to the ureter. With one finger in the pelvic

Case 1. Case 2.

Case 1. Male, aged 39. Pennsylvania Hospita August 8, 05. Intra- and extra-peritoneal uretero-lithotomy and appendectomy.

Case 2. Female, aged 32, Pennsylvania Hospital August 31, 05. Intra- and extra-peritoneal uretero-lithotomy and appendectomy.

cavity I was able to push the ureter and stone up into the extra-peritoneal wound within easy reach, but not within sight. I then opened the ureter longitudinally and removed a long, black stone, about the size of the end of the little finger and cucumber-shaped. In order to remove the stone quite a large opening was made in the ureter. A gauze drain was then passed down to the opening in the ureter and the peritoneal cavity was tightly closed with a continuous gut suture. The upper portion of the wound in the rectus was closed with sutures through its fascia. The superficial tissues were closed excepting at the lower end of the wound, where the gauze drain made its exit. In spite of the flow of urine through the gauze, which was quite profuse for about two weeks, the abdominal wound healed without any infection. On the 2nd of September very little urine was discharged from the fistula, and on the 6th of September the fistula was closed. It opened later, however, for a short time, but closed again, and has remained closed. The patient since his operation has worked as a laborer and has had no further attacks of pain.

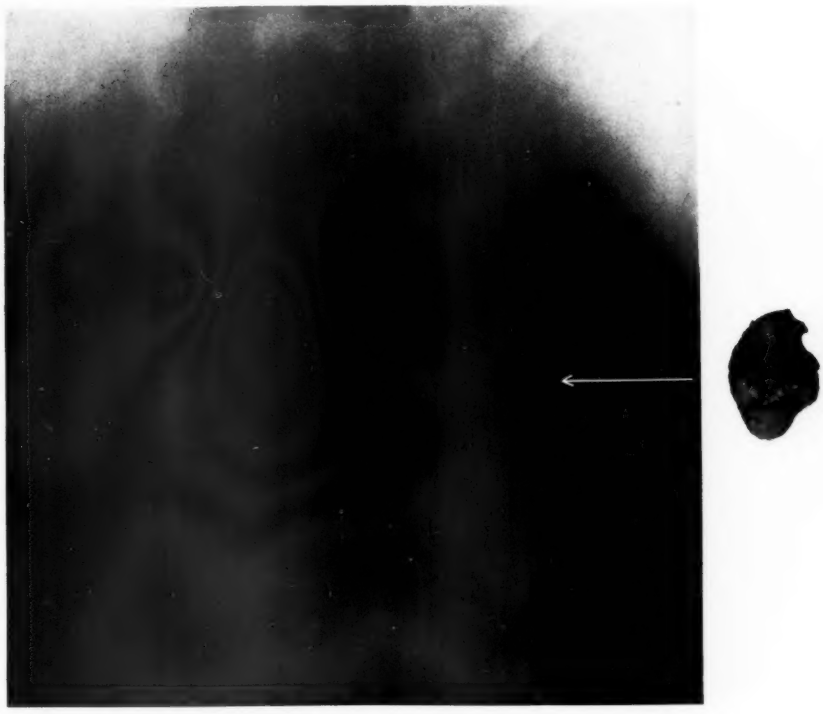
Case 2. A domestic, aged 32, was admitted to the Pennsylvania Hospital, August 12, 1905. This patient was sent into the hospital at night as a case of appen-

[1] Read by invitation before the Suffolk District Medical Society and the Boston Medical Library, February 5, 1908.

dicitis and was seen in my absence by Dr. Francis T. Stewart. Dr. Stewart did not think the case one of appendicitis or one demanding immediate operation. I saw the patient the next day and could not make up my mind that there was any inflammation of the appendix. She gave a history of repeated attacks of pain in the right side of the abdomen in the appendiceal region, accompanied by vomiting. The pain was more or less fixed, there being no radiation to the back or to the bladder and there were no urinary symptoms. The examination of the urine, however, on the day after admission showed a few red-blood corpuscles and a trace of albumin. Repeated examinations of the urine continued to show red blood corpuscles. Examination of the abdomen on the day after admission showed considerable tenderness over the right kidney. This, however, disappeared and the only tenderness remaining was in the iliac region. A few days later, the patient passed both blood and mucus by the bowel; and the cæcum and sigmoid were quite tender. These symptoms did not persist, as did the microscopic blood in the urine. A differential diagnosis between appendicitis, colitis, renal calculus and ureteral calculus had to be made. With the disappearance of the renal tenderness and of the blood and mucus in the bowel movements, and in view of the absence of the characteristic symptoms of appendicitis, I made a diagnosis of a probable ureteral calculus on August 30th, and advised operation. The diagnosis was based on the continued tenderness in the iliac region, the repeated attacks of pain, accompanied by slight rise of temperature, and especially the persistent microscopic blood in the urine. Urinary symptoms were absent in this case, as in the previous one; there was no frequency of micturition, and no pain in the bladder. The right kidney was not movable. The menses had been irregular and painful. I determined to follow the same technique as in the previous case, especially as I was in doubt regarding the diagnosis. Therefore, on the 31st of August, under ethyl chloride-ether anæsthesia, the abdomen was opened through the outer edge of the right rectus, low down, the appendix was readily found and removed as in the previous case; it was perfectly normal. The ureter, which was thickened, was easily felt crossing the iliac vessels. About one inch below the vessels a hard, small, immovable mass was felt. The peritoneum was stripped away from the abdominal and pelvic walls after increasing the incision downwards. The ureter was exposed and brought plainly into view. Great assistance was also derived in this case from a finger within the pelvis pushing the ureter up into the extra-peritoneal wound. The ureter was incised longitudinally and a small irregular stone, very rough and adherent to the ureteral mucous membrane, was removed. A gauze drain was inserted down to the ureteral wound, the peritoneal cavity was closed, and all but the lower portion of the wound in the abdominal wall. Although in this case a smaller incision in the ureter was required than in the previous one, there was a greater and more prolonged leakage of urine. The wound healed promptly excepting at the point of drainage, and the patient never had an abdominal symptom. On September 30th, one month after the operation, there was no flow of urine, but there was some discharge from the drainage tract which remained open for about two weeks. The patient was discharged on November 29th, the wound having remained closed for about a week. The patient was heard from recently and has remained perfectly well since her operation.

CASE 3. Single woman, aged 50, admitted to the Pennsylvania Hospital September 19, 1906. For some months she had noticed a gradual enlargement of the abdomen, but supposed it only an accompaniment of advancing years. During the previous two weeks there had taken place considerable swelling of both lower extremities. This was quite marked on admission, and there was a distension of the superficial abdominal veins and those of the buttocks and thighs. There was a large fluctuating tumor in the right side of the abdomen extending from the pelvis up to the border of the ribs. It seemed most prominent in the right iliac region. The rest of the abdomen was soft and pendulous. Rectal examination revealed nothing definite at this time, and vaginal examination could not be made because of a tight hymen. There was present a mass of inflamed hemorrhoids. The patient had no fever or other indication of an inflammatory condition. She had had no urinary symptoms. By those who admitted her it was thought she probably had an ovarian cyst, and it was only when I examined her on the operating table that I concluded that the tumor was a distended kidney: because of the swollen extremities and distended veins I thought it likely that the growth might be malignant. The patient was very much emaciated and her condition was not particularly promising for an extended operative procedure. The tumor was so large that I concluded to approach it through the peritoneal cavity. It was found to be an enormously distended kidney with a dilated ureter. The peritoneum over the tumor was divided and the kidney removed after being emptied with a trocar. Six and a half quarts of dark brown fluid were withdrawn. This was the most marked hydronephrosis I have ever seen. Upon withdrawing the collapsed sac there was no difficulty in isolating all the structures at the hilum and ligating them separately. The vena cava was exposed for about six inches. I removed with the kidney about 6 inches of ureter. I was so well pleased at getting the kidney out without more difficulty, and I was so impressed with the patient's poor condition at the time of operation, that I foolishly did not explore the remaining ureter. The edges of the posterior peritoneum were united to the edges of the anterior peritoneum and the wound closed excepting for a gauze drain. The colon had been pushed to the median line by the growth, so that it did not incommode us in any way during the operation.

The patient made a very satisfactory recovery from the operation excepting that she had at times irritability of the bladder and for a while had difficulty in passing urine. The irritability of the bladder continuing, the patient was anæsthetized with chloride of ethyl and both a careful vaginal and rectal examination

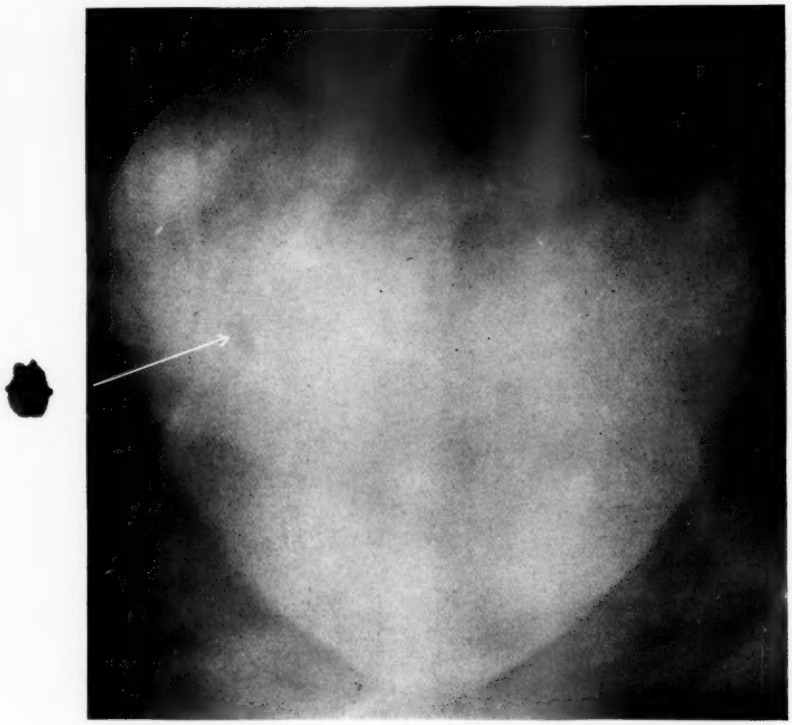


Case 3. Female, aged 50. Pennsylvania Hospital, October 26, 1906. Extra-peritoneal uretero-lithotomy.

made. I then detected a hard mass at the lower end of the right ureter. I had already sounded the bladder for stone but could find none. A skiagraph was made by Dr. W. E. Lee, which showed the stone within a short distance of the bladder.

Six days after the previous operation she was again anæsthetized and the ureter exposed through an extra-peritoneal incision made just above Poupart's ligament. Passing down through the floor of the inguinal canal the peritoneum was pushed away until the stone could be easily felt. It lay within a very short distance, probably an inch or half an inch, of the bladder. The removal was greatly facilitated by an assistant, who passed a finger into the rectum and pushed the stone up. The ureter was opened and there was an escape of considerable pus. The stone is shown in the accompanying illustrations. The ureter was quite friable and easily torn. I passed my finger down to the vesical orifice and up to the point where it was ligated in the the previous operation, but no other stone was felt, although the one removed showed a distinct facet on one end. The X-ray plate had revealed but one stone. There was considerable bleeding when the ureter was first exposed, apparently venous in character. It was controlled by pressure, however, and when the operation was completed there was no bleeding. A gauze drain was inserted and the wound partially closed. The patient made a most satisfactory convalescence but there was a considerable discharge of pus from the wound for some time. Since leaving the hospital she has been perfectly well and her weight has increased from 104 to 147 pounds.

Case 4. Married woman, aged 32, operated on at the Bryn Mawr Hospital, June 11, 1907. I saw this patient first with Dr. Thomas F. Branson, of Rosemont, Pa., on May 23, 1907. She had had her first attack of severe abdominal pain about three years previous. It lasted a very short time and was not severe. The pain was rather general, though more marked on the right side. A diagnosis of appendicitis was made. She was operated upon by another surgeon two months later; the appendix, which apparently was perfectly normal, at this time was removed. She had no more pain for a year. Since then, however, she had had repeated attacks of typical renal colic, the pain being largely in the back and in the left loin. Dr. Branson had only had charge of the patient for three or four months, and during this time she had had several attacks. The pain radiated down the ureter in each of these, and there was always considerable tenderness



Case 4. Female, aged 32, Bryn Mawr Hospital, June 11, 1907. Intra- and extra-peritoneal uretero-lithotomy.

over the ureter after the attacks. Several X-ray plates had been made previously of the kidney and upper portion of the ureter, but all of them were negative. Catheterization of the ureters had also been done without detection of the stone. In one attack the patient passed considerable blood during its subsidence, but during the recent attacks there had been only microscopic blood. An X-ray plate made by Dr. Willis F. Manges, showed a distinct shadow about the size of a pea, in the line of the left ureter just below the brim of the true pelvis. Examination of the patient showed the right kidney to be quite movable. Neither kidney was tender. There was, however, considerable tenderness over the left ureter. The X-ray plate indicated that the left kidney was larger than the right, and lower down than it should be, but I could not demonstrate this clinically. The patient stated that in her attacks she had passed large quantities of urine, so that this enlargement might have been due to a blockage of the ureter. I did not operate upon this patient until June 11th. On the day previous to operation another X-ray plate was made which showed the shadow in exactly the same position. A small, round shadow was shown in this second plate, just below the other, which had not shown in the previous plate. Dr. Manges thought that it was probably a phlebolith.

The patient was anæsthetized with the chloride of ethyl-ether-morphia sequence. An incision was made about one and a half inches internal to the anterior superior spine and parallel with Poupart's ligament, and the muscles divided in the direction of their fibres. The peritoneum was separated from the pelvic wall up as far as the bifurcation of the common iliac. There was considerable bulging of the sigmoid into the wound, and I had difficulty in detecting the ureter. A small incision, sufficient to admit one finger, was then made in the upper part of the exposed peritoneum, and through this opening I readily detected the calculus about one and a half inches below the iliac vessels. Before discovering the calculus, a round body below the ureter, which felt too soft for a calculus, was discovered, and determined afterwards to be a gland; I think it was this which cast the shadow in the second plate. The calculus was pushed up into the exposed portion of the ureter, and was very easily brought into view. Supporting the ureter with the forefinger in the peritoneal cavity and the thumb in the external wound, it was incised longitudinally and the stone

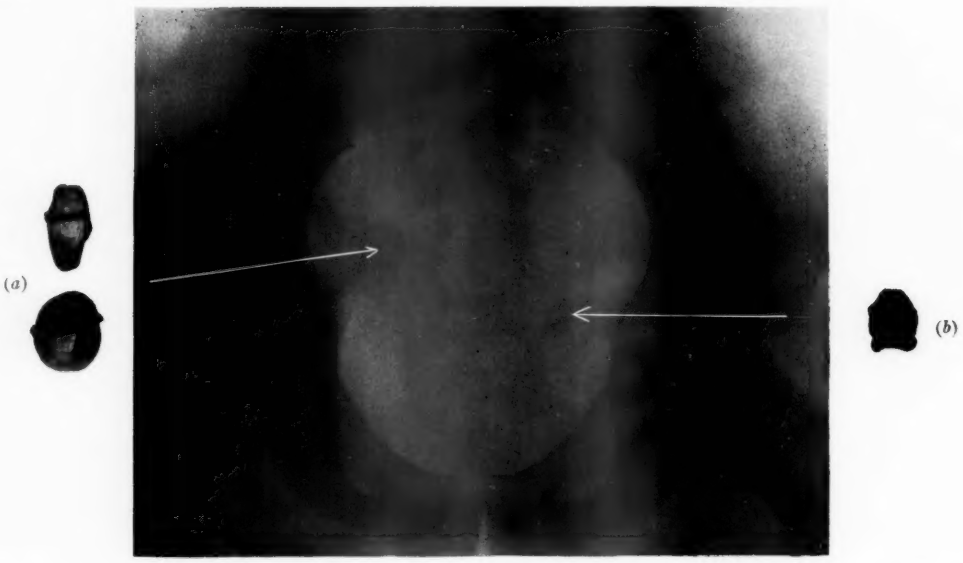
Case 5. Male, aged 12. Jefferson Hospital, October 12, 1907. Intra- and extra-peritoneal uretero-litholomy.

removed. It was oblong and resembled a split pea: one end was quite rough with projections. No other calculi could be felt in the ureter, although it was explored thoroughly. The peritoneal wound was then closed and the ureteral wound closed with several sutures of chromic gut. A small gauze drain was introduced down to the site of the sutured ureter, and the wound closed. The drain came out in the middle of the incision, and a subcuticular stitch was introduced on either side of it.

In this case there was a very satisfactory exposure of the ureter, and no difficulty at all in removing the stone and closing the wound. The gauze drain was removed on the fifth day. The wound healed promptly, and there was never any infection or leakage of urine.

This patient has been free of all attacks of pain since the operation. She has put on weight, and excepting for occasional backache, which she had had long before her renal attacks, and for which a suspension of the uterus had been done, she is perfectly well.

Case 5. Boy, aged 12. Operated upon at the Jefferson College Hospital, October 12, 1907. I saw this boy about six months before his operation with Dr. E. E. Graham; at that time he gave a typical history of attacks of renal colic with blood and pus in the urine; the attacks, however, were never very severe. Several X-ray plates were taken at different times by Dr. W. F. Manges, and always showed a fairly good sized stone lodged between the pelvic brim and the bladder. At that time, I recommended operation but it was declined. Dr. Graham kept in touch with the case, however, and as the symptoms continued the patient was again admitted to the hospital. He had continued to have blood and pus in the urine. A recent X-ray plate showed the stone a little lower down, apparently, than in the previous plates. It was stated that the boy had also passed blood and mucus by the rectum, for which he had recently applied to the Rectal Department. Whether there was any association between this condition and the ureteral calculus it is difficult to say, but it was interesting to note that in my second case this same symptom was present. As the stone was quite large, I felt I might be able to locate it easily without opening the peritoneum. An incision was made just above Poupart's ligament, the muscles separated, the peritoneum pushed away along the iliac vessels, and an attempt made to locate the stone. This failed and a buttonhole in the peritoneum at the lower angle of the wound near the inner edge was made. The forefinger was introduced and the stone at once located about an inch and a half or two inches below the iliac vessels. The peritoneum was further separated from the pelvic



Case 6. Male, aged 10. Pennsylvania Hospital, January 15, 1908. (*a*) Intra- and extra-peritoneal uretero-lithotomy; *b*) Extra-peritoneal uretero-litholomy.

wall down to the point of the stone, and with the forefinger in the peritoneum and the thumb outside an attempt was made to force the stone up the ureter so that it could be brought to the surface and the ureter opened at a healthy point and in clear view, but this we were unable to do. There was considerable thickening and old inflammatory adhesions about the ureter. Rather than separate it from its surroundings I concluded it would be wiser to incise the overlying ureter without seeing it. This I did by fixing the stone with the forefinger and thumb, as in the previous cases, and carrying down a pair of curved scissors over the iliac vessels. The opening was made in the long axis of the ureter and just long enough to permit the extraction of the stone with forceps. The stone was rough and very adherent to the mucous membrane. It was nearly round and measured half an inch in diameter. There was little or no bleeding following the extraction. No sutures were introduced into the ureter, but a gauze drain was carried down to the site of the wound. The forefinger was then withdrawn from the peritoneum, and the wound in this structure closed with catgut. The muscular and skin wounds were partially closed. The patient passed considerable bloody urine for several days, but there was never any leak from the ureter. About ten days or two weeks after the operation, when the boy was up and about, he had some pain with a rise of temperature, and passed several pieces of gravel about the size of the lead of a pencil. Since this time he has been perfectly well. His external wound measures $1\frac{3}{4}$ inches in length.

Case 6. Boy, aged 10 years, operated upon at the Pennsylvania Hospital, January 15, 1908. This boy first began to have attacks of pain in the right side of the abdomen, with bloody urine and frequent micturition about two years ago. Since this time he had had numerous attacks of sharp pain lasting for several days, accompanied by the passage of considerable bloody urine. He came into the hospital during the subsidence of one of these attacks, which had lasted for several days. At the time of admission there was marked tenderness along the course of the right ureter and over the bladder. There was very little tenderness over the right kidney and none on the left side of the abdomen. The bladder was sounded for stone but none found. Two X-rays plates were made by Dr. A. E. Billings, with an interval of several days, and each showed stones; on the right side there were two stones, one smaller than the other, and the two very close together in the line of the ureter. On the left side was a small oblong stone. One plate showed a small shadow some distance from the stone in the region of the left ureter, but not in the line of the ureter, but as the other plate did not show this shadow I was not inclined to believe it was a stone. The stone on the left side was lower down than the two on the right. By rectal examination the stones on the right side could be just felt with the tip of the finger, and no stone could be felt in the left ureter, although the stone on this side was lower down. By confusing the two sides of the X-ray plates, we thought the large stones were on the left and the smaller stones on the right

side, but the operation convinced us of this error. The right ureter was exposed extra-peritoneally by the muscle-splitting incision parallel to Poupart's ligament and just above the inguinal canal. The peritoneum was pushed away from the pelvic wall until the ureter was reached, but it was impossible without considerable rough handling to get the ureter and the stones up to a point where they were readily accessible. The stones seemed fixed at a point about 1½ inches below the iliac vessels. A small buttonhole incision was then made in the peritoneum, near the upper border of the wound, and a forefinger inserted. The stones were readily found. I then tried to locate the stone in the opposite ureter through this incision, but could not do so without increasing my wound to an extent which I did not wish to do, as it was my intention any way to explore the other ureter. Returning to the right ureter, I was able with my forefinger inside the peritoneum and my thumb in the extra-peritoneal wound, to free the ureter and get it up into view. The stones were held with the forefinger and thumb and a longitudinal incision made through the ureter and the stones removed. No sutures were inserted but a small Penrose drain was carried down to the ureter. The peritoneal wound was then closed and the muscular structures partly closed. A corresponding incision was made on the opposite side and the peritoneum separated in exactly the same way. I had no difficulty in feeling the stone lodged about 2 or 2½ inches below the iliac vessels. The stone was quite freely movable in the ureter; and I was able to pass it up to a point about one inch above the iliac vessels and bring the ureter well into view; without opening the peritoneal cavity. The ureter was incised, the stone removed, and two gut sutures inserted. A Penrose drain was introduced and the wound partially closed. The entire time occupied in this operation, including both sides, was thirty minutes. The patient made an excellent recovery, the drains were removed on the third or fourth day, there was never any leakage of urine, and in two weeks the patient was up and moving about the wards, with the wounds entirely healed. For the first 24 hours he passed some bloody urine but not as much as during his last attack. The wound on the right side measures 3 inches, and that on the left side, 2 inches in length.

A brief survey of these six cases shows that in the last four an absolute diagnosis was made on the clinical symptoms and X-ray presentations. In the second case, ureteral calculus was suspected, and in the first case it was not suspected but found in operating for appendicitis. In two cases the stone was felt through the rectum or vagina. The ureter was sutured in 2 cases (4 and 6). In the last four cases there was no urinary leakage whatever. The combined intra- and extra-peritoneal uretero-lithotomy was done five times and the extra-peritoneal uretero-lithotomy was done twice. All the patients recovered.

PATHOLOGY

The occurrence of stone in the ureter is a much more common condition than was ever thought until the X-rays revealed its frequency. Leonard (1) and other radiologists state that ureteral stones are probably more frequent than renal stones. This statement, of course, has hardly been borne out in our operative experience: it nevertheless deserves serious consideration, and we believe that further experience will tend to corroborate it.

Ureteral stones may be either primary or secondary. Primary stones must of course be very rare, and can occur only when there is some localized point of irritation or ulceration of the ureter, or where a foreign body is present. A silk suture, for instance, may be the nucleus of a primary ureteral stone. This occurred in a case operated upon by Kelly at the Johns Hopkins Hospital, and included in a report of cases by Schenck (2). The secondary stones are the common ones, and make their way from the kidney. These stones pass into the ureter when they are small and may become lodged and greatly increase in size.

Location. There are three locations in the ureter where a stone is apt to become lodged. The first is at a point about an inch and a half below the pelvis of the kidney; the second, is in the bend of the ureter about an inch to two inches below the pelvic brim; and the third is the vesical portion. Many authorities, including Morris (3), state that a common point of lodgment is where the ureter crosses the brim of the pelvis. A careful examination of the reported cases, however, and my own experience, tend to show that most of the stones are lodged below the iliac vessels and therefore, below, and not at, the pelvic brim. Schenck, in one of the first exhaustive papers on this subject, reports a collection of 101 operations for ureteral calculus. The situation was noted in 98. In 20 the calculus was within 6 cm. of the kidney; in 14 at or near the pelvic brim; in 55 within 6 cm. of the vesical orifice; in the remaining cases the stones were distributed at other points in the ureter. Morris reports 44 cases in which the location was stated. In 19 the stone was lodged within 2 inches of the kidney; in 13 "just before or where the ureter passes through the

Case operated upon at the Pennsylvania Hospital, October 10, 1906. Shadow shown is that of a cyst in the broad igament containing material of the consistence of putty.

vesical wall"; and in 11 somewhere about the line of the brim of the true pelvis; in 3 of these 11 the stone was below the brim, in 2 on the brim, and in 5 just above the brim. Tenney (4) has made a collection of 134 cases; adding 33 to those of Schenck. Morris states that impaction at the vesical orifice is apparently more common in males than in females. The earlier operations were done for stones lodged near the kidney, and at the vesical orifice; and consequently the impression arose that these were the most frequent sites of lodgment. Recent X-ray reports, however, tend rather to show that the portion of the ureter between the pelvic brim and the vesical portion, measuring about 4 inches, is a very common site for ureteral calculi. Morris attributes lodgment at this point to the curve made by the ureter. The lodgment of a stone in the ureter may be due to the narrowing or constriction of the ureter or to the size, shape and degree of roughness of the stone. Of course, as said before, an ulcer of the ureter, or a foreign body, such as a suture penetrating its calibre, may determine the site of development of a primary stone.

Size and Shape. Apparently ureteral calculi may be of any size or any shape. My own few cases show a great variety in both. Bovee (5) reports a case in which he removed by the transperitoneal route, a stone weighing 1310 grains and measuring $2\frac{3}{4}$ x $1\frac{1}{2}$ x $1\frac{1}{4}$ inches. The ureter was sutured and vaginal drainage established. There was no leakage. The majority of stones are of an oblong shape, especially those which are primary or which remain and increase in size after lodgment in the ureter. In my fifth case, however, the stone was quite large, and nearly round, and must have been in the ureter for a long time. Israel and Morris each report cases of stones measuring 5 or 6 inches in length.

Number. In the majority of cases operated upon but one stone has been found in the ureter; but on the other hand, the ureter has been found packed with stones for nearly its entire length, and the term "chain-stones"

Case operated upon at the Pennsylvania Hospital, November 12, 1906. Shadow shown is an appendiceal concretion.

has been applied to this condition. Doyen, quoted by Schenck, removed 24 stones from the ureter by the vaginal route; Morris has removed as many as 9 stones from the ureter; Young (6) removed 3 large calculi from the lower end of the ureter by the extra-peritoneal iliac route; and Kelly (7) removed 2 stones from one ureter by the transperitoneal route. In one of my own cases (Case 6), two stones were removed from one ureter and one stone from the other. Fiori, quoted by Deaver (8), removed 10 or 12 stones, making an incision in the ureter 16 cm. long, for the purpose. The incision was closed and the urinary leak which followed ceased in a few days, and the patient recovered.

Passage. Larger stones pass through the ureter than one would suppose. The smaller ones may do little damage to the ureter in their passage, but the larger ones often produce ulceration, especially in the vesical portion of the ureter. Leonard is a strong advocate of the non-operative treatment of many of the small stones which apparently are lodged in the ureter, his opinion being based on the idea that these stones will pass. Of the dangers of leaving a stone impacted in the ureter something will be said later.

Results of Impaction. Many reported cases go to show that a stone may remain impacted in the ureter for a number of years, and apparently create but a small amount of discomfort. The size and shape of the stone in my first case, and the fact that the patient had been able to carry on his work as a laborer with but little discomfort, tend to illustrate this statement. It will be observed, however, in this case that the stone was smooth. As a result of the impaction the ureter may become ulcerated and thickened. It may be perforated by the stone, as in Keen's case (9), and cause an abscess around the ureter, or it may perforate into the rectum or bladder. In Keen's case the stone was lodged just below the pelvic

brim and was definitely located by the X-rays. It was removed by the extra-peritoneal route, the patient making a good recovery. The ureter above the stone in most instances is dilated, the degree of dilatation of course depending upon the degree of obstruction. In my third case the ureter was as large as a thumb. Even if a stone should pass on into the bladder, it may produce sufficient ulceration of the mucous membrane to cause a stricture of the ureter. A stone lodged in the ureter tends to increase in size, and its rapidity of growth will probably depend upon the degree it interferes with the passage of urine. It is quite probable also that the lodgment of a stone in the ureter tends toward the production of other stones behind it in the ureter or kidney. A rough stone, if unattached, will interfere less with the passage of urine than a smooth one, but many of these rough stones become firmly embedded in the mucous membrane and completely obstruct the ureter.

Probably the most serious changes resulting from the impaction of ureteral calculi occur in the kidney. A *complete* obstruction of the ureter by a stone causes an atrophy of the kidney with complete loss of function. An *incomplete* obstruction of the ureter produces a hydronephrosis. Although a hydronephrosis is the first result of an incompletely obstructed ureter, it is very apt to be converted into a pyonephrosis, especially if there are a number of stones present. A complete anuria may result from the lodgment of a ureteral calculus, and this calls for the promptest treatment.

Deaver reports a case in which a carcinomatous change had taken place in the ureter at the point of lodgment of the calculus. I have been able to find no other similar case, but it goes to show one of the possible results of allowing a stone to remain impacted for any great length of time.

SYMPTOMS AND DIAGNOSIS

The symptoms of ureteral calculus may be typical and quite sufficient for diagnostic purposes, but this is by no means the rule, for they are often confusing, and although indicating stone somewhere in the urinary tract, do not indicate even approximately its location. It may be said that, aside from the use of the X-rays and possibly the ureteral catheter, it is impossible to differentiate between stone in the ureter and in the kidney. Stones in the lower portion of the ureter and especially in the vesical portion, may be so confused with stones in the bladder that a diagnosis can be arrived at only by the use of the sound, the cystoscope and the X-rays. Stones lodged within two inches of the bladder may be palpated through the vagina or rectum. Unless a stone can be palpated, detected with a ureteral catheter, seen with the cystoscope, or definitely shown in an X-ray plate, operation is hardly warranted unless clinical signs are persistent and then operator should expect to find some other lesion than calculus. It is certainly not warranted until all the means mentioned, and especially the X-rays, have been employed.

Pain and Tenderness. Pain, the result of ureteral calculus, is nearly always of a paroxysmal character. In most cases operated upon, there is a history of repeated attacks of colic. Occasionally, as in Case 3 of this series, the patient will be unable to recall an attack of colic, and yet the ureter may be completely obstructed and the kidney destroyed. The pain so closely resembles that of renal colic when the stone is in the upper portion of the ureter that it is nearly impossible to differentiate between the two; and when it is in the vesical portion or near to the bladder the pain and other symptoms may so closely resemble those of vesical calculus as to make differentiation impossible without the use of the X-ray, the sound, or the cystoscope. It is probably true that in nearly every case of renal colic there is a partial or complete passage of a stone into the ureter. When the stone has lodged in the lower portion of the ureter, the pain is very apt to be referred to the bladder and the testicle, and occasionally to the glans penis. Frequent micturition and tenesmus are common accompaniments in these cases. Parker (10) has reported a case of a large calculus, removed by suprapubic cystotomy in which all of the pain and tenderness were referred to the rectum. Localized tenderness is of far more value in the diagnosis of ureteral calculus than the character or site of the pain, but often this may be misleading, as for instance, in Case 4, where the tenderness was greatest over the bladder,

and was accompanied by marked vesical symptoms. Localized tenderness is much more indicative when found after the subsidence of the colic. The tenderness, due to stones lodged in the lower ureter, is apt to be much more localized and fixed and persistent than the tenderness due to an inflamed appendix. The difference in the tenderness, however, of the two conditions is not sufficiently great to enable one to make a differential diagnosis without the consideration of other symptoms. We are all familiar with the urinary symptoms that accompany many cases of appendicitis where the appendix lies against the ureter or where there is an inflammatory effusion into the pelvis. The tenderness also can be easily confused with that resulting from an inflamed ovary or tube. Vaginal examination and history are usually sufficient, however, to differentiate the two conditions. The size of the stone does not influence the pain so much as its shape and degree of roughness. For instance, in Case 1, where there was a large, nodular, but smooth stone, the pain was much less than in Case 4, where there was a small, rough stone. In conclusion it may be said regarding pain that where there is a renal colic associated with marked vesical symptoms, a calculus in the lower portion of the ureter should be strongly suspected. Backache is a rather common symptom of ureteral stone, but it cannot be differentiated from the backache due to renal calculus.

A vaginal or rectal examination should be made in every suspected case and where the stone is within two inches of the bladder it can as a rule be felt. Cases have been reported in which a stone could be felt by palpation through the abdominal parietes, but this was not true in any one of the cases here reported.

Hæmaturia. Hæmaturia when accompanied by other symptoms, and especially when persistent even to a slight degree after the subsidence of the attacks of pain, is of great significance. I think that the persistent microscopic blood in the urine for several days after an attack of colic is of more value than marked bleeding during an attack. In order to avoid error the urine examined should be passed voluntarily, as otherwise it cannot be said that the blood is not the result of some traumatism produced by catheterization. The blood obtained through a ureteral catheter, especially, cannot be relied upon as having been produced by a stone, since the catheter itself may easily produce bleeding.

Polyuria, Oliguria, and Anuria. Polyuria coming on with the subsidence of an attack of pain is of great significance, especially if it has been preceded by an oliguria during the pain: it would tend to show that the ureter had been blocked, urine had accumulated behind it, and that now the blockade had been lifted and that the dammed up urine was making its escape. Complete anuria has frequently resulted from blockage of a ureter by a stone, and when this condition accompanies and follows an attack of renal colic, calculus should be at once suspected and operation promptly instituted.

Urinalysis. Daily urinalyses are of great value and should be especially directed to the discovery of microscopic blood. Some authorities have stated that they can tell more or less definitely by examination of cells the point of the urinary tract which is diseased or injured. This, however, has not been placed upon such a basis as to be of real diagnostic value. The same may be said of cryoscopy. Pus is often present, especially where the stone is embedded in the ureteral wall or where it occupies the vesical portion of the ureter. None of the urinary findings compares in value to that of persistent microscopic blood.

Fever and Chills. In the majority of my own cases a rise of temperature has accompanied most of the attacks of colic, and occasionally a chill has preceded the rise. Serious infection of the ureter, however, is not indicated by the rise of temperature. Repeated chills should cause us to suspect serious ulceration or abscess formation.

Cystoscopy. The use of the cystoscope may throw much light on the diagnosis of ureteral calculi. The benefit to be derived from the use of this instrument, in cases where the stone is lodged in the vesical portion of the ureter is at once apparent. Numerous cases are on record where a stone has been seen projecting through the ureteral meatus and where its removal has been easily accomplished by means of the cystoscope. Young reports several such

cases. Even where the stone cannot be seen, much may be learned by a comparison of the ureteral openings and the character of the urine passing through them. In several of the earlier reported cases of ureteral calculus the bladder was opened and the calculus palpated in the ureter some distance from the vesical orifice.

Ureteral Catheterization and Exploration. Schenck states that in 6 cases at the Johns Hopkins Hospital the stone was demonstrated and definitely located by the wax-tipped catheter. Young also has located them with the ureteral catheter. Fowler (11), too, reports a case in which the stone was located with the catheter and subsequently confirmed by X-ray and operation. Personally, I have had no experience in this line. In two of the cases (4 and 5) of my series, ureteral catheterization had been done without giving rise to any definite information. The catheter, apparently, in both cases passed the stone. If the ureter be dilated behind the stone, the latter may be easily displaced by the catheter which will then pass. That ureteral catheterization is not without its dangers, even in healthy ureters, is shown by a case of Noble's (12). In this case, which was operated upon in 1901, a stone in the ureter was apparently indicated on an X-ray plate. Noble had, a few days before operation, catheterized both ureters without difficulty. He determined to reintroduce the ureteral catheters for the purpose of aiding in the location of the ureter during the operation. The catheter met with a slight obstruction and very little force was used to overcome it, when the catheter passed quite readily. An incision was then made for the extra-peritoneal exposure of the ureter in the iliac region. The peritoneum was accidentally opened, and the opening was then increased in size and a finger introduced for the purpose of palpating the ureter. The operator was suprised to find the ureteral catheter free in the pelvic cavity, having perforated the ureter just outside the bladder wall. The ureter was closed, no stone could be found, although careful search was made, and the patient recovered without leakage of urine. Noble reports another case of successful extra-peritoneal removal of a calculus from the lower ureter, in which the ureter was sutured and there was no leakage. Tilden Brown suggested the introduction of stilletted catheters into the ureters before X-ray plates were made, in order to determine the relative position between the ureter and any shadows which might be found. Kolister and Schmidt (13) recommend the passage of soft flexible wire probes, made of lead blended with antimony, for the same purpose. Either of these methods would, in our opinion, carry with them far more danger to the ureter than would be warranted by the results obtained. If a healthy ureter can be injured as easily as in Noble's case, how much more readily might an ulcerated ureter, or one blocked by a stone, be perforated. The passage of an instrument through any long, inflamed or ulcerated mucous tract is always a dangerous procedure. The ureteral catheter may be valuable in determining the performance of function of the kidneys and ureters, yet this information usually may be obtained by the simple use of the cystoscope. The wax-tipped catheters may also prove of service where the X-rays fail to reveal a stone, and yet the clinical signs point persistently to this condition. In this connection, it must be remembered that the X-rays in the hands of experienced men at the present time, practically never fail to show a stone, unless it is a pure uric acid or phosphatic stone, and even these are often shown.

Roentgen Rays. It is due nearly altogether to the X-rays that ureteral surgery has reached its present stage of usefulness. Leonard and others deserve high praise for the work they have done in this line. The question at once arises, can the X-ray plate be relied upon when it shows no stone? The answer to this question is yes, unless the stone is a pure uric acid or phosphatic one, and such stones we know are very rare. Leonard (14) claims to show even pure uric acid stones. In nearly every case of uric acid or phosphatic stone oxalate crystals are also present in sufficient degree to cast a shadow. Osgood (15), in an excellent paper on the Diagnosis of Obscure Cases of Renal and Ureteral Calculus, reports a case of large uric acid calculus in which no shadow was cast in a number of otherwise good X-ray plates. My own feeling is that the X-rays are much more apt to deceive us by showing

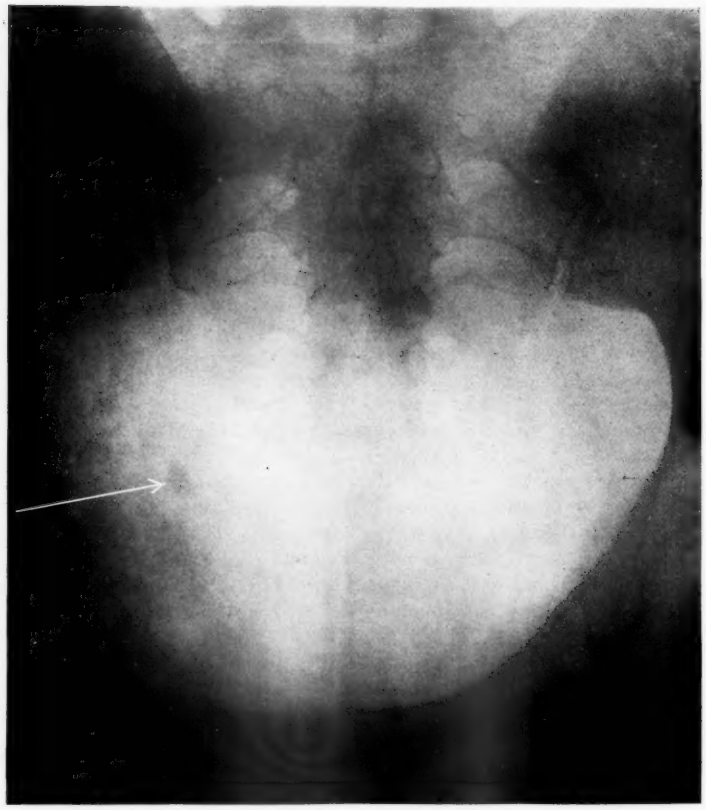
a shadow which is not due to a stone, than by showing no shadow when a stone is present. Brewer (16) has reported a case in which an appendolith cast a distinct shadow; and we have had a similar experience. A print of the plate in my own case is shown. The patient had an appendiceal abscess overlying the ureter, with marked vesical symptoms, including at first retention, followed by hæmaturia and great vesical pain. A mass developed and a diagnosis of appendiceal abscess was made. An X-ray plate was then made which showed a distinct shadow in the line of the ureter and the patient was operated upon for ureteral calculus with probable peri-ureteral abscess. When the abscess was opened through a muscle-splitting iliac incision, an appendolith was found free in an abscess cavity and a necrotic appendix formed a part of the abscess wall. The patient recovered and has been entirely free of all symptoms since his operation, which was done November 12, 1906. Radiologists tell us that appendoliths do not cast shadows, but there are certainly exceptions to the rule. The present concretion was placed under quite a muscular arm, and a distinct shadow was again shown in the plate. In another case, the X-ray prints of which are shown, and in which I strongly suspected a ureteral calculus because of the shadow shown in the line of the ureter and the associated urinary symptoms, I found on opening the abdominal cavity for exploratory purposes a small, tense cyst in the broad ligament filled with a putty-like material. The ureter was carefully examined but no stone found. Frequently small round bodies are shown on X-ray plates of the pelvic region which are irregularly placed; these are believed by radiologists to be phleboliths and when they occur in the line of the ureter, are extremely confusing. Calcareous deposits in muscles and vessels also cast confusing shadows. It is in such cases that ureteral catheterization with the wax-tipped catheter, or intra-peritoneal exploration may be indicated if the urinary symptoms are marked. In order to be of the greatest value, the X-ray plate should include both kidneys, both ureters and the bladder. Failure to carry out this rule has often resulted in serious accident. In one of Deaver's cases stones were removed from one ureter, and at postmortem it was found that the other ureter had been blocked by a stone which had not, before the operation, been suspected. In one of my own cases (Case 6) the stone in the left ureter was not suspected, nor were there symptoms pointing to it. In view of the experience had in this case, namely the confusing of the two sides of the plate, it is important to fix definitely the right or left side. I would suggest placing a penny, or some other metal object, on the right anterior superior spine of the ilium. This would render such confusion of the two sides as occurred in this last case impossible.

Stereoscopic radiographs are of additional value, and give one a fairly good idea of the relation of the stone to surrounding bony structures. In Case 4 Dr. Manges made such plates and the result was most satisfactory. Before an X-ray plate is made, a careful preparation of the patient is absolutely essential. The bowels should be thoroughly evacuated, preferably with castor oil and enemata. If this rule is not followed, confusion and errors are sure to result.

Although of probably the greatest value in diagnosticating ureteral calculi, the evidence of the X-rays alone is not sufficient. The other signs and symptoms must be considered with the X-ray plate. The interpretation of the plate itself also is frequently difficult, even in the hands of an expert. It is always well to take more than one plate, and especially to have a plate made within at least forty-eight hours of the time of operation. If this is not done, a fruitless and unnecessary search of the ureter may be made, for a stone which has passed into the bladder.

TREATMENT.

Every case of ureteral calculus does not require operation, but every stone that is too large to pass or that remains stationary for any length of time, or that is giving rise to severe attacks of colic that do not carry it farther down the ureter, and, above all, one that produces symptoms indicating blockage of the ureter with distension of the kidney or infection, as indicated by the urinalysis, should be removed irrespective of its situation. That fairly good sized stones do pass we all know.

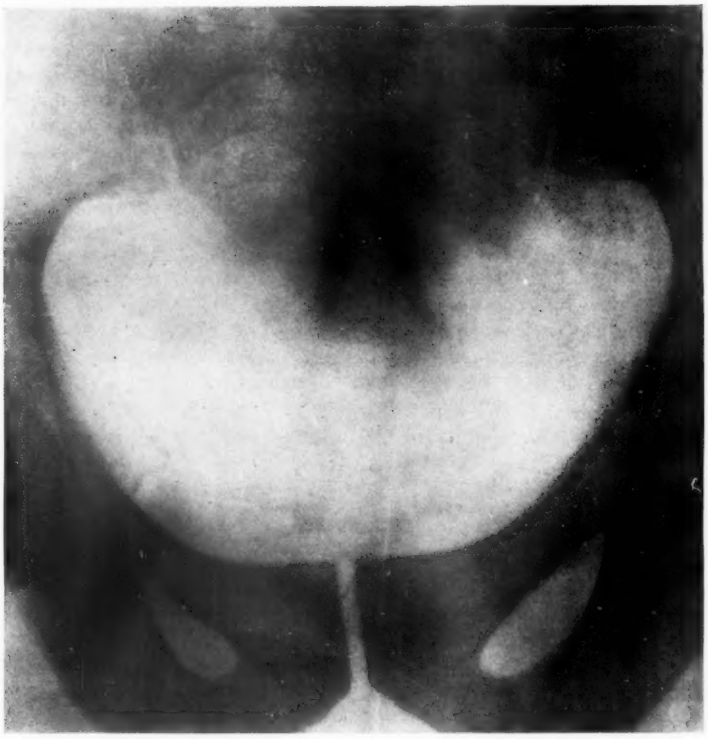


Da Costa's case of ureteral calculus, passed just before admission to hospital for operation.

The accompanying skiagraph of a case under the care of my colleague, Dr. J. Chalmers Da Casta, shows a fairly good sized stone which passed just before the patient was admitted to the hospital for the purpose of having it removed. On arriving at the hospital, the patient said that a day or two previous he had had a sharp pain during micturition, that the urine stopped and then came with a rush, and that he heard something strike the urinal. Another X-ray plate was made, which is also exhibited, in which the calculus is not shown. The drinking of large quantities of water will greatly aid the passage of a ureteral stone, and this treatment has been urged by Leonard and others. It has been suggested by Schmidt that the injection of vaseline through a ureteral catheter would aid the passage of a stone situated in the lower portion of the ureter.

The operative treatment had best be considered as applicable to stones situated in the upper third, in the lower two-thirds, and in the vesical portion of the ureter, as entirely different operative procedures are required in the different localities.

Stones in the Upper Third. Stones in the upper third may be removed through a lumbar incision, such as is usually employed for the exposure of the kidney, or through the Israel incision, extending from the tip of the last rib obliquely downward, along the iliac crest. The operation should be performed retroperitoneally. When the stone is located, the question will arise as to whether it shall be



Da Costa's case after passage of the stone.

pushed up into the kidney pelvis and removed here, or through the kidney substance. Deaver has advised pushing the stone into the pelvis and removing it through the kidney cortex, and this was the advice of Israel, Tuffier, Newman (17), and others. More recent work, however, has shown that it is perfectly feasible to remove the stone through a longitudinal incision in the ureter, and this I should much prefer, even if the stone were movable. If, however, it is freely movable and can be easily passed back into the pelvis and the pelvis is quite accessible without an increase in the length of the incision, it may be removed through a pelvic incision. Clean wounds of the pelvis heal promptly whether sutured or not. We are inclined to believe that a wound in the ureter, whether sutured or not, will heal more quickly and with less likelihood of a urinary leak than one in the pelvis, and experience and recorded cases bear out this view. In the absence of a suspected renal lesion, incision through a healthy kidney for the purpose of removing a stone which has been lodged in the ureter is not advisable.

Stones in the Lower Two-thirds. It is with the stones situated in the lower two-thirds of the ureter, and especially in the lower one-third, that this paper is most concerned. The pelvic portion of the ureter has but recently come into the field of legitimate surgery. Young states that as late as 1898 Fenger asserted that the lower ureter was beyond the reach of the surgeon. But a short time later, however, it was shown by numerous case reports, to be a field where most successful surgery could be done. Although there are several methods of approach to this portion of the ureter, there can be no question at the present time that the iliac extra-peritoneal

route is the best. It is well, however, to consider the other methods of removal.

The *transperitoneal exploration* of the ureters may be of the greatest value, and, in certain doubtful cases, clearly indicated. The removal of ureteral stones through the peritoneum is not to be recommended as it does not compare in safety with the extra-peritoneal uretero-lithotomy. Kelly was one of the early operators in this field of surgery, and removed successfully two stones by the transperitoneal route. Desquin, quoted by Schenck, has also been successful in a transperitoneal operation. Steward (18), in 1900, discovered a stone in the ureter near the bladder by palpation through the bladder wall after doing a suprapubic cystotomy; some weeks later he opened the abdomen and removed the stone through the peritoneum. The ureter was sutured and the patient recovered without any leakage of urine. Notwithstanding this and other successes, it must be only in occasional instances that one would be warranted in doing a transperitoneal uretero-lithotomy.

The *vaginal* route may be justifiable where the stone is low down and easily felt, and especially where abscess formation is threatened. This route, however, is open to numerous objections: one of these is the great danger of uretero-vaginal fistula; another is the inaccessibility of the rest of the ureter for exploration; and still another is that the stone may slip up out of reach and great injury to the ureteral wall be necessary in order to recover it. In the recorded cases of vaginal operations many more fistulæ have occurred than in the peritoneal or extra-peritoneal operations.

The *rectal* route we believe should never be employed unless an abscess has formed and rupture into the rectum is imminent.

The *perineal*, or *pre-rectal*, route is an old one which has been successfully employed on but very few occasions. It has no advantages over the other operations and is accompanied by much greater difficulty and uncertainty.

Two *posterior operations* have been recommended for the removal of stones occupying the lower ureter. Cabot (19), who was one of the early workers in this field, in 1892 recommended a modified Kraske operation for the exposure of the ureter. Henry Morris more recently has practised in the female a posterior operation: the incision is a long one, parallel with the border of the sacrum and coccyx and extends as far down as the anus. Any posterior operation must have few advantages, and it certainly has serious objections. The wound is always a painful one, it is apt to become infected from the rectum, which itself may be injured, and the opportunity of examining any extent of the ureter is limited. The opening in the ureter is made in the floor instead of in the roof, which we believe is more apt to result in a urinary fistula. The posterior operations also require more mutilation than the anterior.

The *extra-peritoneal iliac* route is the one of choice by practically all surgeons at the present time. Among the first surgeons to use this method successfully were Israel, Twynam, Tuffier, Morris, Finney, Young and Keen. Bardenhauser was probably the pioneer, although his operation in 1882 was a retroperitoneal one for stone in the upper third of the ureter. The iliac extra-peritoneal method of approach became established about 1900. Young states in his paper, published May, 1903, that there had been done but nine iliac extra-peritoneal operations for stone in the lower ureter in the male by five operators. The surgical literature since this time has contained numerous case reports illustrating the great utility and success of this operation. In Tenney's 134 cases of ureteral stone, there were 34 extra-peritoneal operations with 5 deaths; 3 of the 5 were suffering from anuria when operated upon; one had cystic kidneys; and the fifth a dilated and fatty heart. Much of the popularity of the operation is due to the report of Twynam's (20) case. In 1889 he operated upon a boy, 8 years of age, who had had frequent attacks of pain radiating to the head of the penis and accompanied by hæmaturia. The abdomen was opened and both ureters were examined through the peritoneum. A stone was found below the pelvic brim on one side. The abdominal wound was then closed and the operator conducted a number of experiments on the cadaver in order to decide upon the best means of removing the stone extra-peritoneally. He later performed the iliac extra-peritoneal uretero-lithotomy suc-

cessfully. Rolfe and Godlee (21) about the same time reported a successful case of iliac extra-peritoneal uretero-lithotomy in which the ureter was not sutured. Lister and Godlee (22) had reported a case a few years previously of much the same kind in which the stone was successfully removed extra-peritoneally. Betham Robinson (23) has reported a successful extra-peritoneal uretero-lithotomy for stone in the lower third of the ureter in a three year old child.

The incision employed by the various operators has differed but slightly. As a rule, it has been parallel with Poupart's ligament, and has extended downward for 3 or 4 inches from the anterior superior spine. It is much the same incision as that made for the extra-peritoneal ligation of the iliac vessels. With the exercise of ordinary care it is unlikely that the peritoneum will be injured, as it can be readily separated from the iliac vessels and pelvic wall. The ureter always adheres to the peritoneum when the latter is separated. The exposure of the ureter is most satisfactory, excepting in the portion which lies behind the broad ligament in the female. A stone situated in this position, however, can be passed up into the exposed portion of the ureter and here removed. Some operators have advised crushing a stone so situated between the fingers and thumb and forcing it into the bladder. This we believe to be fraught with considerable danger to the mucous membrane of the ureter and would not recommend its practice for fear of subsequent ureteral stricture. There is practically no hemorrhage accompanying the operation, unless the stone is situated behind the broad ligament; there is little or no damage to important structures; and if the muscles have been separated in the line of their fibres and not crosscut, there is practically no danger of hernia. Another advantage of this route is that in case it becomes necessary, because of lesions in the ureter or kidney higher up, to expose this portion of the urinary tract, the incision can easily be carried up into the loin and the necessary exposure obtained. A small opening can be made in the peritoneum and valuable information and assistance obtained in doubtful or difficult cases. In opening the ureter the incision should be made in its roof, and as so many cases have been reported where no sutures have been introduced, and yet no leakage has occurred, we are inclined to believe that sutures are unnecessary. A small gauze drain should be carried down to the ureteral wound but gauze should not be packed against the ureter, as it rather invites a fistula. If no urinary leak takes place in three or four days, the drain can be removed. The introduction and maintenance of a ureteral catheter beyond the wound in the ureter for the purpose of avoiding a leak, has been carried out on several occasions, but it is an unnecessary procedure in the uncomplicated case. The passage of a ureteral sound or catheter to the kidney and to the bladder, in order to determine whether or not there are other stones or strictures in the ureter is a wise precaution. Force, however, should never be exercised in this exploration and if a stricture is found it should be dilated or incised. Such strictures are most apt to occur in the vesical portion of the ureter and can usually be dilated through a cystoscope. Young has reported a case of this kind. Sampson (24) has performed extra-peritoneal uretero-lithotomy under infiltration anæsthesia.

The *combined intra- and extra-peritoneal* route offers certain advantages over the simple extra-peritoneal operation. Its greatest field probably lies in those cases where the abdomen is opened for some other condition or for exploratory purposes, and a stone is found in the ureter, as occurred in Case 1 of this series. So much help can be obtained in the removal of a stone from the ureter by having a finger or hand in the peritoneal cavity, and so much less traumatism to the ureter is done, that when the peritoneum is open I should not think of closing it until the stone had been removed through the iliac extra-peritoneal incision. As has been shown in the cases reported, it is unnecessary in many instances to make a separate incision. The peritoneum can be easily separated from the wall of the abdomen and pelvis and the ureter exposed. At the time I performed my first operation, which was August 8, 1905, I did not know of any operator having previously followed this plan. I have since discovered, however, that Erdmann (25) on March 11, 1905, accidentally opened the peri-

toneum in doing an extra-peritoneal iliac operation, and then intentionally enlarged the opening, confirmed the diagnosis, removed the appendix and then the stone, just as was done in my first two cases. T. C. Witherspoon (26) in 1904 explored the ureter through a peritoneal incision made through the rectus muscle. A stone was located, the peritoneum was closed, and the ureter exposed extra-peritoneally through the same incision and the stone removed. Hall (27) in 1890 reported a case in which a stone, 3 inches below the pelvis of the kidney, was discovered on exploratory laparotomy. The kidney was then exposed posteriorly and the stone pushed up and removed through the kidney incision. Cabot, in 1892, spoke of the advantages of intra-peritoneal exploration of the ureter in doubtful cases. One of the chief advantages of the combined method, as practiced in my own five operations, is the ability to explore thoroughly the entire ureter on the suspected side and a large part of the ureter on the opposite side. Another advantage is the fact that it is unnecessary for exploratory purposes to separate the ureter from its surroundings to any great extent and that damage to it through rough handling is much less likely. The stone can also be much more readily passed up into a healthy portion of the ureter in the combined than in the simple extra-peritoneal method. The one objection to this method which can be raised is that of possible infection of the peritoneal cavity. By the exercise of certain precautions I believe this can be reduced to practically nothing. My own rule has been not to open the ureter until the stone has been thoroughly exposed and the ureter fixed in the position desired for incision; and that when the forefinger has once been removed from the peritoneal cavity it is not again inserted. When the stone is removed the sutures should be introduced at once or a gauze drain passed down to the ureteral opening. The peritoneum is then closed and the gauze drain brought out at a point as distant as possible from the peritoneal wound. In none of the reported cases of transperitoneal removal of ureteral calculi has there been any infection of the peritoneum. This, of course, does not justify the transperitoneal removal of the stone in the minds of most of us, but it does tend to show that even here infection of the peritoneum is not common, and that the dangers from infection in the combined operation, as was carried out in the cases reported, is very small indeed.

In certain cases it is difficult to locate the stone, and sometimes to find the ureter by the extra-peritoneal route without extensive dissection and a prolonged operation. In such cases an opening in the peritoneum, sufficient to admit the forefinger, greatly reduces the time of the operation and saves extensive dissection. Attention is again called to Case 6, in which two stones were removed from one ureter by the combined operation, and one stone from the other ureter by the extra-peritoneal route, in 30 minutes; and this was not the result of a habit of operating hurriedly, but rather the result of the easy access to the ureter through the described incisions. I do not wish to be understood as advocating the opening of the peritoneum in every case, nor have I practiced this, but in doubtful or difficult cases I do believe that it is a measure which will greatly facilitate the operation, and reduce its complications, especially injury to the ureter. It is unnecessary to repeat the advantages to be derived from the ability to thoroughly explore the entire urinary tract through such an opening.

Stones in the Vesical Portion. Stones in the vesical portion of the ureter may be removed by the extra-peritoneal route, by the vaginal route, by means of a suprapubic cystotomy, by first dilating the female urethra and the use of the finger or forceps, or through the operating cystoscope. Young has shown that the extra-peritoneal route is feasible in these cases and he also demonstrates their removal through the cystoscope. The vaginal route has been a very common one for removing these stones in the female. Garceau (28) has presented an interesting communication on this operation. The vesical portion of the ureter is easily accessible through the upper portion of the anterior vaginal wall; and probably the greatest objection to the operation is the fact that the stone may slip back into a dilated ureter and make its removal difficult without considerable injury to the ureter itself. In case strictures or other stones

are detected higher up in the ureter, access to them through the vagina may be difficult or impossible. Urinary fistulæ following the vaginal operation have not been infrequent, and I believe largely so because the incision is made in the floor of the ureter. In operating through a suprapubic cystotomy wound, through a dilated female urethra, or through the cystoscope, the stone may be grasped in case it projects into the bladder, and thus easily removed; or the ureter may be dilated or incised, and the stone removed with forceps. In all of these operations it is good practice to prevent the forcing of the stone further up in the ureter by digital pressure upon the stone by a finger passed into the vagina or rectum.

The rectal route for stones lodged in the vesical portion is not to be recommended unless an abscess has formed, and perforation is threatened.

In closing I would urge the careful exploration of the ureters in operations for chronic or subacute appendicitis and when on opening the abdomen, no lesion is found sufficient to account for the symptoms.

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THE CÆSARIAN SECTION FROM THE STANDPOINT OF A PERSONAL EXPERIENCE[1]

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WITH the great activity which has been displayed in the development of the Cæsarian section during the last few years, there seems to be considerable danger that the operation may be over-exploited in the next few years. Its wise use must depend upon the proper selection of appropriate cases and upon the rejection of cases which are for any reason unsuited for it. A consensus of opinion upon these points can be reached only by the combined work of many men and the final judgment of the general profession thereon. To this growth each of us can but contribute his personal opinion and experience, and as such a contribution this paper is intended. I feel that I must apologize for the somewhat profuse use of the personal pronoun in the first part of it but I was not able to see how I could avoid this in a paper based upon personal experience and at the same time make the points which I wished to make without tiresome circumlocution. I shall cheerfully accept any criticism of literary style which may result from this if I can succeed in making my points more clear.

The methods of practice which I wish to present to you are based upon beliefs that the mortality of the Cæsarian section varies so greatly in accordance with the period of labor at which it is performed that — First — The mortality of the section late in labor is too great to permit of its performance in the interests of the child alone. Second — The maternal mortality of the section performed even so early as at the end of the first stage in labor is greater than that of any ordinary high forceps or version. Third — The mortality of the section done at the time of election in advance of labor, or at its very beginning — The Primary Section — is so low that it is a safer operation for both patients than difficult high forceps or version when performed for mechanical obstacles.[1]

I am necessarily excluding from consideration cases in which either orm of operation is performed for other than mechanical obstacles.

These principles are founded, first, upon my own experience, second, upon the results of a constant inquiry into the difference between the mortalities of late and early Cæsarian operations, which I have been interested in keeping up in my own reading for more than ten years, and, lastly, and I think least valuable, upon a statistical inquiry into the general literature of the last ten years, which I have recently made and published[1] and shall quote again here.

My first point must therefore be a brief statement of my own experience and of the results of this statistical study.

My own experience with the section now comprises thirty cases, all without mortality. It has been one-sided, since it includes but one case which could by any possibility of construction be considered late, and that patient, though she had been through a very long first stage, was in good condition and showed no sign of exhaustion. It has been thus one-sided because an analysis which I made and published in the early 90s before I had myself done the operation, and which I think included all the cases which had then been done since the revival of the operation, had convinced me even at that early date that the mortality of the section was chiefly confined to infected and late cases. I believe it to be because of this one-sidedness that I have an unbroken record of success. I have, nevertheless, noted even in my own experience that the smoothness of convalescence, the comparative absence of discomfort to the patient, and the absence of anxiety to myself have been marked in the cases done before labor, and in the other cases have varied in proportion to the extent of labor permitted before operation.[2]

My own experience and general studies of the literature of the subject have alone been suffi-

[1] Bost. Med. and Surg. Journ. Oct. 31, 1907, and Trans. Amer. Gyn. Soc., V. 32, 1907.

[2] I have subjected cases seen late in labor to the other operations, including even craniotomy, and in a few cases symphysiotomy.

[1] Read before the New York Academy of Medicine, February 6, 1908.

cient to bring me to a firm belief in the propositions which I have stated above and am endeavoring to support, but since this is necessarily less convincing to others, I have recently obtained the reports of all the cases in the world's literature which the officials of the Boston Medical Library were able to find for me, and have made and published[1] an analysis of all the cases operated upon in the last ten years by operators who had done at least five sections and which were published in sufficient detail to permit their classification into the necessary three classes of cases; those done before labor, early in labor, or late in labor; which three classes I have ventured to call primary, secondary, and late sections. I excluded from the list all cases which were done for other than mechanical indications, e. g., for placenta prævia, eclampsia, fibroid tumors, etc., since these are plainly foreign to an inquiry into the mortality of the section when done for mechanical indications only. I included all other cases and assigned them to their final place in the three classes spoken of before noting their mortality. My completed tables contained 289 cases operated upon by twenty different operators; of these 82 were primary, 158 secondary, and 49 late.

The 49 late cases showed a mortality of over 12 per cent in spite of the fact that I included in this class all cases in which the os was above the size of a fifty-cent piece, or in which even trifling pains had been present for twenty-four hours, thus putting into it a large number of cases which really belonged to a comparatively favorable class.[2]

The 158 cases classed as secondary were all done during the first stage of labor, none had had even six hours of strong pains or twenty-four hours of even trifling pains; they were therefore very favorable cases, yet their mortality was 4 per cent (6 deaths 3.8 per cent).

The 82 cases done before labor, or with the advent of the first pains, yielded one death, a mortality of slightly over 1 per cent (1.2 per cent) and would have been absolutely clear of mortality had I felt at liberty to exclude this one death, as not only due to an accidental technical slip, but as furthered by the incompetency of an assistant during convalescence. It is, however, a death which must be attributed to the operation and must be tabulated as such.[3]

Even including this accidental death the figures above (1, 4, and 12 per cent) are in marked contrast when we remember that the three sets of cases were operated upon by the same operators and under the same conditions, differing only in the time selected for operation.

This statistical inquiry was conducted with the greatest care to prevent its results from being influenced by my pre-existent convictions, in spite of which care they are so strikingly in accord with my personal experience as to have strongly re-inforced my belief in the three propositions which form the burden of this communication, to which I may now proceed.

One of these;— that the classical Cæsarian section performed late in labor is an ultra dangerous operation;— will be received with little question, and for lack of space must be here dismissed with but scant consideration, for which reason, I undertake it first.

This fact, though generally recognized by operators of large experience, has been so little realized by the masses of the profession that it deserves prominent emphasis in every communication upon the subject made to-day. It is safe to say that there is no operator of experience who is not fully aware of the dangers of the late section.[4]

But the occasional operator has not yet grasped it, and the country is still full of general practitioners who look upon the Cæsarian section as an operation which may be resorted to after all other expedients have failed, and of surgeons of little obstetrical experience who are cheerfully ready to perform it under such conditions. The blame is not to them. The blame is with the obstetrical specialists who recognize the dangers of the late section, but

[1] Loc. cit.

[2] The mortality of really neglected cases would be very much higher

[3] The uterus was sutured with catgut, the sutures became untied (it was nearly ten years ago when catgut was not knotted as it is to-day) the patient collapsed, the operator was at a distance, the assistant saw the patient still in fair condition, but pronounced it a mere nervous attack, unworthy of treatment; and the autopsy showed the uterine wound gaping and the abdomen full of blood.

[4] The percentage of late operations is steadily decreasing among operators who are able to publish lists of any length; and although the choice between the Porro operation, the very unsatisfactory divisions of the symphysis, and the disgusting operation of craniotomy is till sub judice for these unfortunate cases, it is safe to say that there is no operator of experience who is not fully aware of the dangers of the late section.

have failed to emphasize them sufficiently in their teachings.

The late section may still be justifiable in some cases, but its mortality has not been lessened by the success of the early section, it is still an ultra dangerous operation, and should always be explained to the family as such. If it were fairly so described it would be but seldom performed. I believe that future progress in the use of the Cæsarian section will consist in an increasing limitation of the late section and a corresponding increase in the use of the early section.[1]

My remaining propositions are, in slightly altered words,— First — That a section undertaken under every surgical advantage in advance of labor is less dangerous than one performed after even a few hours endurance of the exhausting physiology of labor, and under the technical conditions incident to such work. Second — That the section performed after a full test in labor has demonstrated the approaching failure of the natural forces is an operation favorable to the child, but much more dangerous to the mother than the intrapelvic methods of extraction by high forceps or version.

If it is then true, as I believe has been demonstrated, that in cases operated upon by the same operators and under the same conditions the mortality of the section is closely proportionate to the length of labor endured before its performance, there can be no plainer or more obvious deduction than that *when the Cæsarian section is necessary, it is desirable that it should be determined upon in advance, and performed as a primary operation.* Few obstetricians and still fewer surgeons will deny this almost self-evident proposition as a matter of theory. To secure the advantages of its application to practice we must be able to demonstrate the possibility of predicting in advance what the result of labor will be with sufficient accuracy to warrant the performance of the Cæsarian section.

I believe that this is possible in the large majority of all cases. He would be a fool who attempted to deny that doubtful cases exist, probably must always exist, and must be treated as doubtful cases, but this fact should not hamper our action in cases which are not doubtful. There are cases which are clearly Cæsarian, cases which are doubtful and cases for which the Cæsarian need not be considered, but it is all important that we should separate these three classes.

I believe that in the best-trained hands the doubtful class is now a small and constantly decreasing one, and at alll events it is no credit to the profession that for many years its obstetrical members have been content to rest upon the accepted statement — "We cannot predict the results of labor in advance"— and have made no serious effort to attain that much needed capacity.

The primary Cæsarian section is to-day probably comparable in its mortality to the operation for appendicitis in the interval, and there are at least a very large number of cases of mechanical obstruction in which we can to-day predict the result of labor with a sufficient degree of accuracy to warrant us in doing the section at that safe time, upon the same principle on which we do our interval appendectomies, i e., to warrant in us subjecting the patient to a safe operation in order to avoid the probability of a far more dangerous operation a little later.

We all remember the days when a projected Cæsarian section was sometimes forestalled by the rapid spontaneous delivery of the patient before the operator could get to her. Those were the days when the indication depended upon the mere size of the conjugate by measurement. Those days are no longer with us, we have already a much better understanding of the indications, and such cases as these would now be placed in the doubtful class for whom the primary section should not be advised. We can to-day at least divide our cases of mechanical difficulty into the one class in which the question of the probable result in labor lies between a spontaneous and an operative delivery, and the other class in which the question is only between the intra-pelvic operations and the Cæsarian section.

Most operators of experience are now agreed that a primary Cæsarian section is a better

[1] The treatment which should be adopted for neglected cases and the choice between the revolting, but maternally safe, operation of craniotomy; the rather unsatisfactory divisions of the symphysis; and the original Porro operation, for which there is perhaps a future; is an obstetrical question of much interest, the discussion of which would involve too much space to be permitted here.

operation for the patient than a difficult high forceps or version, on the ground that we all know that difficult intra-pelvic operations for mechanical obstacles not only have a large fœtal mortality but are by no means without maternal mortality, and have also a very great maternal morbidity; while the Cæsarian has an equally small maternal mortality, and, speaking broadly, no fœtal mortality or maternal morbidity.

I should be glad to quote briefly a few cases in illustration of my own experience on this point.

CASE 1. Dec. 1895. Mrs. C. S. 30 years. 2-para. First child still after operative labor in another city. A large, stout, rather pale woman, indisposed to exertion, easily fatigued, and easily put out of breath. Normal urine and no abnormal heart sounds. Pelvis probably somewhat scant, but not recognized as "contracted" by measurements. Inclination of brim excessive. The pains were lingering and ineffective and after a somewhat long first stage of labor, both maternal and fœtal pulses rising in rapidity, forceps applied and living child extracted. The perineum which had been repaired after a bad laceration in the first labor was again extensively torn, with free, though not alarming, hæmorrhage from the torn surface. Its repair occupied considerable time during which the patient was held under ether. She had been cleaned up and turned around in bed but a few minutes when I noticed a sighing respiration and great pallor. The respiration rapidly became worse, the pulse was within a few minutes almost indistinguishable, and death occurred in about twenty minutes. No autopsy was permitted. It is possible that there was an embolism, but I think that the death was the result of long labor, prolonged use of forceps, considerable hæmorrhage, the shock of forcible extraction and extensive trauma, and perhaps an unduly long etherization, in an anæmic patient with in all probability a flabby, if not an actually fatty, heart muscle. I believe this case to be an instance of the dangers of difficult intra-pelvic operating, extensive trauma and its repair, in patients who have not been properly watched and prepared for labor during pregnancy. I had even then had a large experience in labor, I hope that my care of this patient was up to the standard of practice at that day, but I believe that this labor could be a great deal better managed at the present time.

CASE 2. May 1905. Mrs. M. F. 37 years. 2-para. Seen with Dr. S. A. Houghton. First child stillborn in operative labor. Referred to me when two months advanced in second pregnancy. Watched through pregnancy and delivered with Dr. Houghton at term.

A large and strong looking woman but with a history of ineffective pains in the first labor, a condition of tonic constriction occurring early. In this first labor the cord prolapsed and stopped. The hand introduced through the os to replace it found the uterus rigidly applied to the child. The child being dead and the conditions difficult it was very wisely decerebrated to facilitate its extraction, which was then easy. Weight ten pounds. Dr. Houghton was confident that had the soft parts been in proper condition a living child could have been extracted by version.

My examination of her pelvis during her second pregnancy was as follows — legs bandy, sacral and lumbar curves excessive, inclination of brim extreme, length of symphysis 7 cm., inclination increased, promontory high, crests low but curves normal, external measurements normal, diagonal conjugate not less than 8.5 cm. Owing to great inclination of symphysis, true conjugate estimated at same size. On examination under ether at term, the adaptation between head and pelvis did not seem excessively difficult; in view of the extreme inclination of the pelvis I was inclined to favor the Cæsarian but in view of Dr. Houghton's wide experience, known skill, and previous experience with the individual pelvis in labor, attached much weight to his opinion that the delivery of a living child by version would be possible. The patient and her husband were opposed to the Cæsarian, and would not consent unless I was ready to assure them that it was the only method which offered any prospect of a living child. After long consideration we decided upon version, and in view of the previous behaviour of the uterus and some features in the condition of the patient, I recommended that it be done as a primary operation. The cervix was dilated with the greatest ease and the child was easily turned but the extraction of the body was exceedingly difficult, the head followed easily. The child was evidently completely exhausted by the exceedingly difficult extraction of the body; it was resuscitated with great difficulty and died after a few hours. The vagina and perineum were extensively lacerated, the perineum through the sphincter, probably during the extraction of the arms, which was very unusually difficult, and the patient had a troublesome incontinence of urine for many weeks after delivery. Although this disappeared spontaneously and I was fortunate enough to secure complete repair of the perineum by a primary suture, neither of these are results which could ordinarily be counted upon, and the net result of the case was a stillborn child.

This case formed a turning point in my obstetric growth. I had then done 26 Cæsarian sections and a very large number of intra-pelvic operations. From the intra-pelvic operations I had had a very few maternal deaths, a much larger number of stillbirths, and a great number of lacerations of a degree sufficient to be rated as furnishing subsequent morbidity, but I had had a very great number of completely successful cases. I had nearly reached the position which I now take, but this case crystalized my views. I am still a very strong believer in the use of the intra-pelvic methods both as primary and secondary operations in mechanically easy cases; but this case is, and I hope and believe that it will always remain, the last case in which I have ever adopted an intra-pelvic operation which I had reason to believe would be

mechanically difficult, at a time when the clean, certain, surgical, primary Cæsarian section was still open to me

CASE 3. Mrs. P. S. Seen with Dr. C. W. Townsend Dec. 6 1899. 3-para. Details of former labors unknown but not markedly difficult. 40 hours in labor when first seen. Head high, os partially dilated, membranes unruptured, no progress for many hours, patient tired. Forceps failed after several applications. On passing a hand into the uterus shoulders held by firm constriction ring. Version performed. Turning difficult, extraction easy. Soft parts very friable, moderate tear of perineum, several small tears of vestibule. Small hematocele developed in left labium. No urine for twenty-four hours, then soft catheter passed by nurse yielded nothing but dark blood. Patient then seen again with Dr. Townsend. Urethra evidently lacerated in delivery. After much study left side of urethra was found to be intact and catheter was passed to bladder. Hematocele between uterus and bladder and larger than uterus. Patient exsanguinated, condition somewhat critical for several days but made gradual and smooth recovery.

Seen again with Dr. Townsend in fourth pregnancy April 1904. Upper part of anterior vaginal wall somewhat œdematous and cicatricial. Pelvis of normal diameter but with much increased inclination of brim and symphysis. Head high and coaptation to pelvis tight. In view of history of last labor recommended Cæsarian. May 4, 1904, Cæsarian performed. Operation easy, recovery prompt and uneventful.

To sum up the objections to the intra-pelvic operations.

Upon the maternal mortality of high forceps and version I have been unable to find any statements which are at once authoritative and recent. It is certainly not high. In a very large personal experience, which I could not now analyze without excessive labor, I am able to remember but two deaths in cases operated upon for mechanical reasons only, and in an analysis of results at the Boston Lying-in Hospital made ten years ago I collected seventy-five consecutive cases with no deaths. A search of the more recent text-books has failed to show any exact statement of modern results, those authors who mention the maternal mortality at all usually contenting themselves with statements to the effect that, though undoubtedly existent, it is also undoubtedly small. It is probably about equivalent to that of the primary Cæsarian section.

The fœtal mortality of these operations is, however, frequently referred to in the same works and in operations done for mechanical obstruction is placed at an average of at least from 20 to 25 per cent.[1]

Their maternal morbidity is plainly not a subject capable of statistical determination, but is fortunately not a subject which needs debate.

Any operator of extended experience who will devote but a short time to a review of his cases will remember many instances in which the resulting trauma was so extensive as to result in morbidities requiring secondary operation, and probably some in which even successful repair was followed by the prolonged or even permanent persistence of some disability. As a gynecologist I have for instance seen and operated on a number of cases of prolonged incontinence of urine after such injuries, though, with the exception of the case quoted above, I have been fortunate in escaping it after my own extractions,[2] and this is but one of many such morbidities.

In contrast, the results of the primary section when performed on healthy women for merely mechanical obstacles are the highest possible degree of safety for the child, a maternal mortality risk which is probably not greater than that of difficult high forceps or version, and an entire absence of the extensive vaginal trauma, and consequent morbidity, a percentage of which we know to be inseparable from the difficult intra-pelvic operations.

For these reasons the primary section is preferable for most cases of mechanical difficulty in which the conditions are such that we can know beforehand with a fair degree of surety that we are going to have an operative labor and that our choice lies between the primary section and the intra-pelvic operations with all their uncertainties.

We cannot, however, always make an assured decision before labor, there will be doubtful cases, and the decision in favor of a primary section is always a responsible one, but that is no reason why we should shirk it. We are here to take our responsibility as surgeons, to stand or fall by the success of our results. I cannot criticise those who still believe that the section after a moderate first

[1] Some authors place it much higher, none lower.

[2] Close inquiry among women will disclose the fact that persistent diminution of ability to control the urine is present after a very large proportion of all obstetric injuries.

stage is an operation with little or no mortality, but he who has once attained the belief that a clean surgical incision and suture under all the advantages of an operation at the time of election is a safer operation than a section in the midst of labor and the exhaustion of labor, and who then from vacillation or timidity deliberaately chooses beforehand to risk that secondary section has undoubtedly failed in his duty.

I am fully aware that if such statements as these attract any attention, their publication involves to the men who make them a certain responsibility for the unnecessary operations which are always performed by half qualified persons during the initial stages of any advance, but I feel that if we can succeed in drawing attention to the enormous mortality of the late section, and the not very small mortality of the secondary section, the lives which will be saved by refusing unnecessary abdominal deliveries under these circumstances will more than make up for the very small mortality which may follow upon the few unnecessary primary sections.

We must next consider the choice between the intra-pelvic extractions and the secondary Cæsarian section.

The secondary Cæsarian section, — i. e., the section performed as soon as possible after the test of labor has demonstrated the mother's inability to accomplish a spontaneous delivery, — has a considerable maternal mortality, (as judged from the statistics quoted above perhaps from 2 to 4 per cent) certainly a higher maternal mortality than the intra-pelvic operations, and an absence of maternal mortality should be our first consideration. I would therefore rule out the performance of the secondary section as an operation of choice; — but here I must explain myself — If called during the first stage of labor to a case in which the delivery of a living child seems otherwise unlikely, I never have and so far as I can see now never shall deny the opportunity of the early secondary section with its safety to the child and its comparatively small risk to the mother to patients who accept its performance in the interests of the child, after they have been warned that it has a moderate maternal mortality, but I would rule it out as an operation of choice for cases seen before labor.

It remains for me to speak in such form as I can of the means at our disposal for the proper selection of cases, but a full discussion of this subject would occupy a monograph, and no small one at that; it is only possible here to outline in the briefest way the principles by which I think that we should be guided.

To attain to any accurate predictions of the probable result of labor demands that each of the three mechanical factors in labor should be estimated with a degree of painstaking care which is to-day seldom practised. The mistakes of the past have been due chiefly to an over-dependence on measurements of the pelvis alone and to over-confident attempts to determine this important question at a single sitting.

To attain really accurate results we must estimate with equal care the *pelvis*, the *passenger*, and the probable *maternal power* as a propelling engine; moreover, to attain any just estimate of any one of these in difficult cases it is usually necessary to see the patient repeatedly during pregnancy, and when it is possible before pregnancy.

In estimating the capacities of the pelvis we must wholly drop the idea that this determination can be made by measurements alone. Pelves vary greatly in shape and consequently in mechanical efficiency even within the normal limits, and of course in pathological examples vary even more widely. They also vary greatly in size. With ample pelves the ordinary variations in shape are of comparatively little moment, but the combination of merely scant normal size with variation, even within the normal limits, towards one of the more difficult shapes, may often make a pelvis in which the birth of a large head may involve really great difficulty. These are the pelves which produce a succession of difficult labors, and often stillbirths, in spite of essentially normal measurements. In the study of the pelvis we must then aim to determine shape as well as size, and we must do this by every means at our disposal.

When it is possible to see the patient in a non-pregnant condition something may possibly be gained by the study of the pelvis with the X-ray provided that one has access to a Roentgenologist who is possessed at once of the electrical equipment and of the high degree

of technical skill which is necessary for this very difficult work. Great care must be observed, however, both in the use[1] and in the interpretation of these skiagraphs, which are often very misleading. The interpretation of the mere curves needs experence, and both the dimensions and the proportions need to be carefully corrected by calculation on optical principles before they can be at all trusted.

The external measurements as ordinarily taken give valuable indications as to the form of the pelvis, but must be supplemented by others, i. e., by measurement of the height of the line of the anterior superior spines above the pubes, of the length of the sacrum and symphysis; and by observation of the inclination of the symphysis, of the inclination of the sacrum in correlation with the lumbar curves, and the inclination of the pelvic brim as seen with the patient in the erect position. These must be checked by observation of the correlated peculiarities of the lengths and curves of the patient's whole skeleton. All this must be supplemented by careful external and internal palpation of the walls of the pelvis on all sides with the trained hand before even an approximate estimate of its shape can be made. Finally we should resort to the one measurment which is diagnostic of size in the ordinary deformities, i. e., the internal conjugate as estimated from the direct measurement of the diagonal conjugate, and should then consider the importance of the size obtained in connection with the shape as diagnosed.

A normal pelvis can usually be determined as such in one visit, but the determination of the amount of an abnormality usually means several interviews, and much study.[2]

The whole matter of the estimation of the shape of the pelvis is as yet in its infancy. It is not now a matter of general professional acquirement, and it probably will not be for many years in a condition in which it can be taught to students or obtained by any other means than the slow training of hand and eye which comes from faithful study of large numbers of cases, but given this pre-requisite it is certain that the knowledge gained by faithful and repeated estimation of a pelvis along these lines is already of a different class from that which follows upon mere measurements.

The probable propelling power of the patient's uterine muscle is of as much importance as the shape and dimensions of her pelvis and the size of her child, that is, a size and shape of pelvis which will permit the passage of a given head with entire safety to the mother after it has been molded by efficient and powerful uterine contractions may be impassable to the same head either by spontaneous delivery or by the intra-pelvic operations in the absence of propelling power sufficient to effect the needed modifications of the shape. It is, moreover, a much neglected fact that the uteri which fail to give obstetrically efficient contractions pass very early in labor into a spasmodic state which annuls their subsequent efficiency, and that the possessors of such uteri uniformly become exhausted early by their inefficient efforts. The study of the propelling power of the uterus is then a matter of the most absolute importance. It may be estimated in advance in two ways. First, and most valuable, by the history of previous labors in multiparæ. In my own experience nothing has been more definite than that the muscular character of labor rarely varies in kind in the different labors of the same woman, though it may be altered in degree by care, or absence of care, of the general condition throughout pregnancy. The woman who has once had strong, successful, sthenic labor will always have propulsive pains. They may be less good if she is allowed to come up to labor in an exhausted condition, but they will always be propulsive pains. Such a woman will overcome any moderate obstacle. Upon the other hand, a woman who has once had a labor characterized by lingering, irregular, ineffective, and excessively painful uterine contractions will never have strong, sthenic, successful labor. Such a uterus may act much better if the muscular and metabolic condition of the patient has been successfully cared for throughout a subsequent pregnancy, but the change will be in degree only. In such women

[1] These high tension skiagraphs involve some danger of burning the patient. It is improbable that a single exposure could interrupt fertility, but this possibility must be borne in mind.

[2] The X-ray can be utilized only in the non-pregnant state, and the estimation of the degree of some forms of deformity can be made best during the non-pregnant state when the balance of the body is undisturbed. The other measurements can be made at any time, but the measurement of the conjugate can seldom be accomplished with accuracy until towards the latter part of pregnancy, unless in very small pelves or with exceptionally long vaginæ; hence the necessity for several examinations in really difficult cases.

exhaustion sets in early, and very moderate mechanical obstacles may be relied upon to prove effective. In multiparæ the history of uterine effectiveness in past labors must therefore be carefully taken and estimated, and before the final decision is reached at the end of pregnancy the history of past effectiveness must be checked by consideration of the patient's general condition then, as compared with that observed before her previous labors.

Even in multiparæ who have been carefully examined, and watched throughout pregnancy, the class in which the choice of operation is doubtful will perhaps still exist, but in them it should be an exceedingly small one.

With primiparæ our power of predicting the probable muscular character of labor is very much less, but even with them it is not wholly negligible. Lack of space must prevent my entering here into a full discussion of a proposition in which I heartily believe, that we can within certain limits predict the muscular character of labor as likely to be propulsive or ineffective, at all events in the more extreme cases, by observation of the woman's progressive loss or gain during pregnancy, more especially since I have published that argument set forth in full to the best of my ability within a year, but even with every advantage in the way of observation a really valuable prediction of the probable muscular character of labor is much less easy in primiparæ than in multiparæ and must be limited in primiparæ to marked cases.

In primiparæ, then, I believe that we must recognize as yet the existence of a considerable class in whom the decision for or against the primary section will remain doubtful, but I believe that the limits of that doubtful class can be somewhat narrowed by careful estimation of the probable character of labor, i. e., by adding to the class who should be left to labor and the intra-pelvic operations, those women of doubtful mechanical obstacles from whom powerful labor may be expected; and by adding to the Cæsarian list, with somewhat more caution, those women with doubtful mechanical indications who have run down in health during pregnancy so markedly that there is but little chance of their having effective contractions. In arriving at the final decision it must be remembered also that women of the powerful, sthenic class are surprisingly little injured in after health by the consequences of labor, but that those of the other or ineffective class endure surgery sufficiently well and labor badly.

Even in primiparæ therefore a cultivated power of estimating the probable muscular character of labor must be one of the acquirements of the thoroughly equipped obstetric surgeon.

Our only reliable evidence of the size, degree of ossification, and, in short, the mechanical qualities of the fœtal head is that obtained by combined external and internal palpation as it is crowded into the pelvis by pressure from above. In cases with marked alterations of the pelvis mere palpation of the fœtus and of the fœtal head without such pressure during the last two months may often show that it is of a size beyond the powers of the given pelvis, but in doubtful cases the decision must be left to a final pressure examination under anæsthesia,[1] and at term, when the head has attained its final characteristics, and when the examination can be pushed to its logical extreme unhindered by the probability that it may induce labor.

In conclusion of this portion of my subject, I believe, then, that to avail ourselves of all the resources of our art in the management of difficult labor we should see the patient at intervals during preganacy and where circumstances render it possible before pregnancy[2]; that the shape as well as the size of the pelvis should be estimated by every possible means at our command, and if necessary by comparing the results of repeated examinations; that the progressive changes in the patient's general condition throughout pregnancy should be watched by repeated medical examinations, each of which should include an analysis of the twenty-four hour specimen of urine, and in the last six weeks of pregnancy palpation of the fœtus; and that in the considerable proportion of cases in which doubt must still remain the decision should be made in accordance with a final examination under anæsthesia at term.

[1] To be described later.

[2] I. e., in cases where the unfortunate history of previous labors or the existence of evident deformity have brought the question of safety of labor into consideration before another pregnancy has occurred.

This examination may be instituted either at or about the calculated date of term, or with the advent of the first pains of labor, in accordance with the opinion of the individual operator. In either case it should be preceded by careful preparation of the vagina and abdomen as if for a total hysterectomy; and the patient should be anæsthetized and placed upon the table with good light, abundance of assistance, and in short all the advantages of modern major surgery. Under these conditions I believe that a competent operator may safely pass his fingers into the vagina, and if necessary within the uterus by dilatation of the os, without compromising the safety of a subsequent section. With the fingers in this position,and if necessary with the half hand in the vagina, he can pass them over practically the whole surface of the head, and with the estimate of its shape, size, and degree of ossification which he can so obtain, aided by observation of the adaptation between the pelvis and head when the latter is forcibly pressed into the brim from above, a competent observer will reduce the mechanically doubtful class to a very small number.

The results of this examination should then be considered in connection with the general information which has been already gained and in multiparæ with the history of past labors. Every obstetrician knows the degree to which this can be depended upon and is aware of the necessity for allowing for occasional exceptions, but the best proof of the pudding is in the eating, and when the actual test in labor has shown such a degree of difficulty in the passage of the bones as to result in even one difficult operative labor with a stillbirth or excessive trauma, the probability of the same result in another labor is so great as to make version or forceps with their uncertainties to both mother and child less advantageous operations for that case than the primary Cæsarian section with its absence of morbidity and probably not greater maternal mortality.

Two provisos must, however, be understood. First,— In pelves which have permitted the extraction of living or even intact children the possibility of a smaller or less well ossified head must not be forgotten in the management of the second labor. The patient's mind should be prepared for either operation, and the final decision should not be made until after a final examination for adaptation, under anæsthesia, and at the time of election for operation. Second,— When the patient has had but one labor, the obstacle which is caused by bony deformity, and that which is due to the rigid soft parts of primiparæ must be estimated separately, and due allowance made for the diminution of the latter in a second labor.

After all these methods have been employed a final decision should be made and rigidly adhered to; i. e., if it is in favor of the Cæsarian this should be at once performed; but, if on the other hand, the case is thought better fitted for labor and the intra-pelvic operations, the patient should be given a full test in the second stage of labor; after which the Cæsarian is the more dangerous procedure and if any operation becomes necessary it must be forceps or version.

The fault in our past management of the Cæsarian operation has been that we have too often adopted or refused it without sufficiently painstaking observation, and also without a full recognition of the superior safety of the primary section. Of course such detailed observation of pregnancy as has been described and such painstaking investigation of the mechanical conditions would be manifestly impracticable, and indeed unnecessary, in the routine practice of obstetrics in multiparæ. It is applicable only to cases in which difficulty can be reasonably expected.

How then should we decide in which cases such care should be used? This is from a practical standpoint the crux of the entire question, and is one which involves the education of the public and the general profession to a realization that the old basis of practice,— i. e., a belief that the majority of labors turn out all right and that the few catastrophes which occur can only be regretted,— it is not in accord with the spirit of modern medicine which demands that even rare, avoidable catastrophes must be foreseen and prevented.

We must teach the general practitioner also that in difficult labor no obstetrician, however skillful, can obtain a low percentage of mortality, fœtal or maternal, if he is only called in after arrest has occurred.

It is not in the least necessary that all labor cases should be referred to specialists, but we must teach the general practitioner how to select the cases in which danger threatens but may be forestalled by an early and accurate comprehension of the circumstances.

All primiparæ ought really to be examined for determination of the mechanical conditions during pregnancy, and the more intelligent portion of the laity would be already prepared for such a routine if the profession were ready for it; moreover, in the case of women with plainly normal conditions that fact is soon apparent, the examination need not be unduly tedious or exacting, and this custom of a routine examination of primparæ is evidently growing in repute — each year certainly brings to my office a larger number of primparæ who are advised to come simply to make sure, and without any real apprehension of difficulty,— but it is not well to try to effect too much at once, and to those practitioners who are not yet convinced of the necessity for a routine examination of all primaparæ, we ought at least to insist upon the necessity for determination of the mechanical conditions during pregnancy in all primiparæ whose general appearance suggests the possibility of mechanical difficulty, and in all multiparæ who have had difficulty.

All primiparæ of very small stature *should have a determination of the mechanical conditions during pregnancy.* It is in this type of women that the symmetrically small pelvis, the pelvis nana, is to be looked for.

All primiparæ who have done heavy muscular work during the period of development, and more especially all of these who present a short, squat, short legged appearance *should have a determination of the mechanical conditions during pregnancy.* It is in this class of women that the flat pelves are particularly frequent.

All primiparæ with the narrow hipped, long straight legged, flat backed, boyish type of figure *should have a determination of the mechanical conditions during pregnancy.* It is in this type of women that the true justo-minor or neutral type of pelvis is especially frequent.

All primiparæ with bandy legs, protruberant buttocks, prominent abdomen, and unusually hollow lumbar region *should have a determination of the mechanical conditions during pregnancy.* Such women are often rather wide hipped, markedly feminine in figure, and at first sight suggest favorable labor; but it is in such women that pelves with excessive inclination of the brim and exagerated curvature of the pelvic axis are mostly found. This pelvis has attracted no attention until of late years. It is frequently of good size and is then unimportant, but its shape is such as to make very slight diminutions of its size productive of an unexpected degree of difficulty in labor, and it is therefore worthy of note. These pelves are the more important in that in them the intra-pelvic operations have necessarily a very high fœtal death rate, and a very high maternal morbidity rate.

All primiparæ of delicate general health *should have a determination of the mechanical and constitutional conditions during pregnancy.* Many of these women have easy labors but in them even moderate mechanical difficulties assume importance, and, moreover, the medical over sight of their pregnancies always demands great care.

Multiparæ who have had easy previous labors demand no special care, but all multiparæ who have had even one difficult or disastrous labor *should have a determination of the mechanical conditions during pregnancy.*

Few men will doubt the wisdom of these propositions when placed before them on paper, but to effect their present adoption in practice is quite another matter. To practically secure this benefit to humanity we must look to our own conduct, we must teach the general practitioner that to seek such an opinion during pregnancy does not mean that the specialist will assume the charge of a normal labor if his examination shows such to be the probable outcome of the case.

It is a fact that twenty-five years ago the care of normal labor by practitioners who were otherwise well equipped, was frequently so poor as to furnish a basis for the position that all labor should be attended by specialists, but this is not the case to-day. With the advent of asepsis and with what I believe to be the general advance in professional equipment and sense of responsibility for results, the average of obstetric practice has risen greatly,

and there are to-day great numbers of practitioners who are quite sufficiently equipped for the intelligent care of ordinary labor, and for the performance of the ordinary obstetric operations. We ought to recognize this change. In all other specialities the conditions of practice have taught us the necessity of not absorbing the management of cases which are referred to us merely for an opinion.

If the obstetrics of the future is, as I believe, to assume a more desirable position as a speciality than it has ever heretofore held, it will be in part because of the change which is coming over our methods of practice; and in part because its practitioners will find themselves not only forced but glad to assume the position characteristic of all other specialists, i. e., those younger obstetricians who desire to take charge of normal labor will probably always take such cases from practitioners who do not attend labor, but I think that established specialists will in the future decline normal labor.

The great majority of general practitioners now desire, and probably will always desire, to attend the ordinary labors which occur in the course of their practice; on the other hand, they are not, and they know they are not, at present competent to make a really thorough determination of the mechanical conditions during pregnancy, It is then setting a very high, and it is to be feared an impossibly high, standard of conduct, to expect them to refer any considerable proportion of their patients, even for an opinion, to men who are their most dangerous competitors for the care of normal labor. When obstetricians are ready to take the posion that under the conditions of to-day attendance on normal cases does not demand a mysterious and unrecognized special knowledge, they will find that the general practitioner is more than ready to seek their advice on the exceptional cases whose results are so apt to be unfortunate, and that he will be ready to seek such advice in advance, or at any other period which the specialist may recommend. The moment this is the case there will be an abundance of work, and of high class work for the professed obstetrician in attendance upon difficult cases whose difficulty has been determined in advance.

PROLAPSE OF THE FEMALE URETHRA

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IN the female urethra it is not altogether uncommon, on the one hand, to find slight eversion of the mucous membrane lining it, more especially in parturient women. On the other hand, it is rare to meet with cases in which the mucous membrane projects to such a degree as to give rise to a distinct bulging or tumour.

A multiparous patient, age 56, came under notice, complaining of hæmorrhage and pain on micturition. The attacks of hæmorrhage were periodic and varied in severity; latterly, an attack, lasting for an hour, compelled the patient to seek advice. She had always suffered from chronic constipation of a most persistent nature and, owing to no less than three attacks of nephritis during the last five years, had been in a feeble state of health. On examination, a red mass, about the size of a large cherry, was seen protruding at the meatus urinarius, which turned out to be the mucous membrane of the urethra, very red, congested, and sensitive to touch. In the middle of the mass an opening was visible from which a clot of blood projected; and a sound, introduced at the opening, passed up into the bladder. Vaginal examination presented nothing abnormal, the cavity being moderate in size and the uterus small and atrophied. The patient was anæsthetised and placed in the lithotomy position, and reduction of the displacement was first attempted. On this being found impossible, the prolapsed mass was removed by scissors, the hæmorrhage, as

might be expected from the vascularity of the mucous membrane, being rather profuse. The mucous membrane was then united to mucous membrane all round the uretheral orifice, a catheter having been previously introduced to keep the canal patent. A dressing was applied and kept in position by means of a bandage, and the patient put back to bed. Two days after the operation she was able to micturate, and ten days later the uretheral orifice had completely healed, and no pain or discomfort was experienced on micturition.

On microscopical examination of a section, made through the whole thickness of the mass which had been removed together with the uretheral orifice embraced by it, it was observed to consist chiefly of angiomatous tissue, which appeared in the tissue as small, separate collections. These angiomatous nodules were, for the most part, enclosed within relatively thick-walled sinuses from the endothelial lining of which they took origin. Each was composed of an intricate network of delicate capillary channels filled with blood, and grew so luxuriantly as in many instances to fill the lumen of the sinus. Frequent hæmorrhages and occasional thrombosis resulted. The intervening tissue was mainly a loose, somewhat cellular, and very vascular connective tissue; but in places it appeared older and more fibrous, with large thick-walled vessels coursing through it. Some of the angiomatous nodules projected slightly beyond the general surface of the mucous membrane, and where this occurred the epithelial covering had disappeared. The overlying epithelium was not therefore continuous but appeared at intervals along the circumference of the section, being abundant in the small depressions between the projecting nodules, but absent from their surface. It was composed chiefly of a transitional type of cell, elongated, tailed, and where most abundant, in several layers. At some places only a few scattered tailed cells could be seen adhering to the surface, while at other parts of the circumference there were traces of squamous epithelium visible.

The causes, detailed by writers on the subject, accounting for such conditions have been ascribed to a sudden fall causing prolapse of the mucous membrane, or a large caruncle dragging on the mucous membrane and causing gradual eversion. Or again, and not at all unlikely in the foregoing case, the constant straining undergone by the patient at stool and a degree of vesical tenesmus, combined with a relaxed state of the mucous membrane due to the general debility of the patient, may have favored, if not altogether caused the condition. Such conditions as cystitis and the presence of a calculus producing more or less continual tenesmus have been known to cause the condition.

Kleinwächter has described two varieties of prolapse of the urethra, one in which the mucosa near the meatus is everted, the other in which it becomes loosened higher up, gradually extending down until it appears at the meatus urinarius.

As regards the progress of such cases, we may have various changes taking place. At first presenting inflammatory changes, they may remain in this state or may at any time become ulcerative or even gangrenous. In some old-standing cases the prolapsed mucous membrane has taken on a skin-like character.

Looking at the symptoms of such a condition, we find them varying to a greater or less extent. In some, no discomfort has been caused, while in others we find the same symptoms present as those characteristic of the caruncle. The prolapse may give rise to or increase already existing vesical tenesmus; and the possibility of cystitis occurring must not be forgotten.

Treatment should first of all be directed to the relief of any causal condition, as cystitis or caculus. One may try to relieve the inflammation of the protruding mucous membrane by the local application of hot water and rest in bed, when simple reduction by manipulation or aid of a sound may be attempted, followed by the use of astringent injections, with a view to promoting contraction of the uretheral canal. This may succeed in cases where there is but slight eversion present, but as a rule some form of operative interference will require to be carried out, as in the case described.

A STUDY OF THE CONDITIONS THAT REQUIRE THE REMOVAL OF THE CHILD FROM THE BREAST[1]

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Part I

THE removal of the nursing babe from the breast may be required for any one of a great variety of conditions which affect the mother or the child, or both.

It may be demanded as a consequence of changes in the milk itself through chemical action, or through the presence in the milk of foreign substances which have been taken in by the mother, or which occur as the result of changes in environment, or changes due to local or systemic disease, or from anatomical variations in either mother or child. In consideration of these many possibilities, it has seemed simpler to take up the subject by an examination of the literature, first in regard to the presence in the milk of drugs administered to the mother, and to follow this with a review of some of the conditions where local or systemic disease demands the removal of the child.

The question of food for the mother during lactation has been the subject of much speculation, and has received more attention, I believe, than it really deserves, for if it is possible to show that drugs and alkaloids when given to a mother rarely affect the milk injuriously, and almost never the babe to a marked degree, it will hardly be maintained that such dilute chemicals as may be found in fruits and vegetables can go over in the milk and affect the child.

The transmission of medical substances through the milk of the mother to the babe is a subject of more than usual interest and more than usual obscurity. Many cases have been reported with the object of putting the phenomenon on a real scientific basis, but owing to the prevalence of tradition and the absence of thorough and accurate methods in collecting the data, the observations are for the most part quite valueless. Furthermore, the reports not only rarely agree, but frequently are so directly contradictory that positive, or even probable, conclusions from analysis are almost impossible.

Under these circumstances, it becomes necessary to pick out the work of a few of the most reliable observers, such as Sonnenberg, Thiemich and others, and therefrom to attempt to formulate a more or less secure working hypothesis.

It is frequently desirable to medicate the mother, without including the child, either for general or local conditions and occasionally to increase or diminish the maternal milk, and it is desirable to know whether this can be done without injuriously affecting the babe.

Another question, at present more or less academic, arises in regard to the possibility as well as the value of administering drugs to the babe by means of the maternal milk. The field is large and interesting; experiments have been few and far from satisfactory.

In making deductions from the evidence, it is always necessary to take into consideration the idiosyncrasy of the patient and to avoid making too sweeping generalizations. One of the most commonly used drugs, and one which for that reason *ought* to furnish a large mass of evidence, is alcohol. Here we have a substance in very general use among all classes of society for its supposed effect in strengthening the mother or babe and increasing the quantity of milk, and yet how few and unreliable are the published reports. Sonnenberg is quite convinced that it passes over in the milk and poisons the child, and describes the resulting nervous symptoms. Thus, a child of some months becomes irritable without apparent cause, fretful, and sleepless; the breast quiets the child only for a time; the period of irritation is followed by a state of deep, almost comatose, sleep, during which the face is occasionally distorted. A free interval may follow, and then a renewed attack, which is co-ordinate in time with maternal excesses. The parents

[1] Read before the Chicago Gynæcological Society, February 21 and March 25, 1908.

blame the teeth, the doctor thinks of meningitis, but many times the condition can be traced to alcoholic excess.

Rouvier reports a pertinent and somewhat corroborative case, in which a wet-nurse took an excess of alcohol; after she recovered from her drunkenness, the nursling, until then thriving and well-conditioned, became irritable, had very prominent red eyes and florid scalp, dry tongue, and all the signs of cerebral congestion, which only diminished after nine days of antiphlogistic treatment. Nothing, however, is said about temperature, bowels, or other possible abnormalities.

Cases have also been reported by Anarion, de Vernay, Meunier, Charpentier, Perier, etc., that are equally inconclusive. Apparently, only two men have investigated the subject with scientific accuracy.

Lewald made many experiments, but could not demonstrate alcohol nor any of its products in the milk.

Klingman used 100 c.c. of alcohol at a dose, and none went over that could be chemically detected. When he used 200 c.c. the animal was overwhelmed, the quantity of milk markedly diminished, but 35 c.c. went over. This latter experiment must be excluded, of course, because the animal was poisoned.

It is therefore, safe to assume, on the basis of this methodical observation, that in general, so far as animal experimentation shows, either very little or none goes over in the milk. That this must be more in accord with our practical experience is shown by the scarcity of published cases in the literature. That in some instances there will be variations from the general law must not only be regarded as possible, but highly probable, and cases should be sought for. Thus Ribemont and Dessaigne et Le Page show that chronic intoxication has a very injurious effect on the nursling, and this they trace to alcohol in the milk, though their method is not unexceptional.

Opium and its derivatives. This drug is highly important, since many cases have been reported where a nursling has met death through the oral administration of a very small quantity. There are authentic cases of infantile death in which a child under five years received only 0.025 grams. Fehling states that a nursing woman can take 0.01 to 0.02 grams of morphine sulphate hypodermically without injury, and that laudanum, moreover, can be given without hesitation, either by mouth or by rectum. Tornhill, after the use of 25 gtts. of tincture opii, finds neither constipation nor sleepiness in the child. Pinzani reports three cases where the mothers took therapeutic doses without effect on either of three children. Cirenville reports a case where the mother took a large dose of morphine sulphate for neuralgia; the next day the child lost appetite, became cyanotic and convulsive, with dilated pupils, and this he attributes to the morphia. It is hard to see how he can make such a deduction from symptoms so contrary to the physiological effect of the drug, unless he can also show that the change in effect is due to some alteration produced by its passage through the body of the mother. Fubini and Cantu report a case that more nearly supports the thesis.

A nurse put a pledget soaked in laudanum into a tooth cavity, and an hour afterward a three months' old child was given the breast. The child became sleepy, lids closed, myosis, prolonged heart and lung motions, and no reaction to irritation. Coffee was given by mouth and rectum, and child recovered in about an hour.

In the experiments inspired by this case, Fudini and Cantu gave 0.02 grams of morphia muriate hypodermically to each of four dogs that had recently whelped. The quantity of milk which they could secure diminished in a few days from 40 to 50 c.c. to 2. c.c. daily, but chemical proof of morphine was never found in the milk. Morphia, however, given to a lactating goat did appear in the milk.

None of the cases can be regarded as free from doubtful conditions and objections. Houselot, on a basis of well-observed cases, considers the question still unsettled, and recommends great care in the use of the drug, and that much less than Fehling permits should be employed.

Atropine, or *belladonna*, is another drug that is used quite frequently in practice as a means or restricting mammary secretions. It is, therefore, a matter of some importance to determine whether or not it passes over to the child, as Sonnenberg claims. Fehling gave

enough to dilate the pupils of the mother, but could get no positive result from the nursling. Fubini and Bonnani, however, in a series of 15 experiments, got positive results which would seem to establish definitely the transmission in the milk of enough atropine to affect the vagus.

Now, Rummo has shown that only 0.00025 grams of atropine is required to affect the vagus, so that Fehling's observation is still valuable in showing that the physiological dose is probably harmless. The statements concerning atropine may be considered as applying to hyoscyamus and its derivatives as well.

It may also be mentioned here that the use of atropine or its congeners as a means of drying up the milk should be dispensed with altogether, since it has been well demonstrated by Storrs that, even if it had such an effect, it is wholly unnecessary, since excellent results follow expectant treatment.

Chloral hydrate. Thiemich denies that chloral hydrate is transmitted to the child, but other writers maintain that it *is* eliminated by the milk, and may exercise a bad influence upon the nursling. Sonnenberg states that the use of 1.5 grams twice daily will produce no symptoms in the babe, unless the breast is given within three-quarters of an hour after the dose is taken, in which case restlessness and prolonged sleep will be noticed.

According to Fehling, chloral should not be prescribed to a nursing mother.

Chloroform. Scanzoni reports a case where for severe after-pains $ChCl_3$ was used for five to six minutes, and the child put to breast three hours later. Being a powerful child, he nursed eagerly, but after a few suctions he dropped the nipple and fell into a deep sleep for eight hours, out of which shaking would not arouse him. He was noticeably restless thereafter for two days.

On this one observation Scanzoni advises against the use of $ChCl_3$ in a nursing mother. No exact chemical proof is at hand.

Ether. Besides the old observation of Goropp Besanse, in which he claims to have distilled ether out of the milk after an ether narcosis, we have the case of Godey. The mother was etherized during labor, and for three days the new-born babe refused the breast. When he finally took it, he vomited all the milk he had taken, and he did this repeatedly, although when put to the breast of another woman he seemed quite content. The two reports are, of course, quite valueless, although interesting.

Quinine is another very interesting drug which is commonly used. Lewin makes the positive statement that the possibility of seriously injuring the nursling through the transmission of quinine in the milk of the mother is proved by experience. He says that the excretion of quinine through the mammary glands has been definitely and correctly determined, but he does not give the methods employed. It is recommended to draw the milk from the breast three hours after the administration of the drug, that the babe may not receive the milk that is richest in quinine.

Transmission of quinine is not accepted by Thiemich without further proof; in fact, he does not regard the reports as in any way plausible. On the contrary, Burdel and Oui admit the possibility, but the former finds it occurs very irregularly. He (Burdel) says that it occurs most rapidly when the stomach is empty, and may become very dangerous to the child, but if used at mealtime, very little goes over in the milk; and the greater the age,the less sensitive the child. Thus, babes of six months or more have little to fear. If one uses quinine in a recently delivered case, it should be employed in conjunction with food.

Salicylic Acid. Pauli first put this drug on a basis free from exception, not alone for the acid, but for its sodium salt. In the use of free acid he constantly obtained positive results, and in a series of 12 observations only once failed. The amount employed rose to 12 grams daily, and the excretion always began within two hours, and ceased soon — only once so late as twenty hours.

The quantitative estimation of the amount excreted, according to the colorimetric method, indicated that the concentration of salicylic acid in the milk did not exceed 0.005 to 0.01 per cent, so that a toxic or even a medicinal dose for the nursling is not to be feared.

Pauli's observation was confirmed by Fehling who got a reaction from the child's urine, and by Stumpf, who used a goat in his experiments.

Potassium and Sodium Bromide were used by Rosenhaupt in an epileptic woman, and he was able to demonstrate chemically that the bromine passed over in the milk. This is not dangerous, however, nor especially to be avoided, since only an infinitesimal amount passes over. The reports of Loughlin, Löwy, and others are also confirmatory.

Antipyrine was found by Pinzani in the milk of seven women, and in the urine of nineteen babes whose nurses were taking the drug in therapeutic doses (Sweisinger method employed). In one litre of milk there was 0.05 grams of the drug when 4 grams had been given within sixteen hours. Quantity and quality of the milk was not affected. Lewin, on the contrary, believes that the quantity of the milk is diminished by the use of the drug, and for this reason should be employed with caution.

Fieux confirms Pinzani's observations, and concludes:

1. Antipyrine certainly passes over in the milk in a natural state.

2. Two capsules of 1 gram each, given at intervals of two hours, may be detected in the milk in from five to eight hours, and in from nineteen to twenty-three hours afterward it cannot be discovered; elimination lasts about eighteen hours.

3. Antipyrine during this time passes into the milk only in excessively small proportions — less than 50 parts in 1,000, and it is only under exceptional circumstances (60 grs. in 16 hours) that it perceptibly reaches this proportion.

4. It does not in any way affect the quality of the milk, lactose, caseine, or fat.

5. It does not affect the quantity of the secretion.

6. The infinitesimal quantity absorbed by the nursling does not seem to have any unfavorable action.

Phenacetine. Lewin states that it is excreted through the urine, not as unchanged phenacetine, but as phenetidin. Acetphenetidin goes over in the milk, and also the derivatives of the same, which is important from a therapeutic standpoint.

Mercury. This is a very important drug because of the frequent indications for its use. According to the older investigators (Peligot, Chevallier, Henry, and Harmer), it was not possible to demonstrate the presence of mercury in the milk of their control animals.

Schaünstein and Späth were unable to get a reaction after four days of inunctions.

Lewald was the first to demonstrate mercury in the milk of the goat, but only after he had given large doses of calomel (4 grams per day), and produced ptyalism and almost a cessation of the milk.

Sigelas found that mercury was eliminated by the milk, but only after a number of days.

Jegourel reports confirmatory observations.

Time for elimination depends on the preparation, the dose, the animal, and the age. The delay in its appearance doubtless explains the failures of many of the older observers. The absorptive power of the leucocytes and other cells may also help to explain the delayed appearance.

v. Klink and others have healed syphilitic nurslings through treatment of the mother, a method which the experiments of Sigelas have now shown to be on a sound basis, although the direct treatment is more rapid and certain, and should on this account be preferred.

Iodine and its derivatives stand in close relation to mercury. It is very readily excreted by the milk. The tincture is excreted within twenty-eight hours after its use. Lewald finds sodium iodine in the milk four hours after a daily dose of 2.5 grams is used. Fehling used a daily dose of potassium iodide, 0.15 grams, but found no influence on the child, but in larger doses it quickly appears in the urine and fæces.

In Lazanski's case a nursing woman took 0.5 grams of KI, and the reaction appeared in the urine the same day, in the urine of the nursling the next day.

Koplick demonstrated a potassium exanthem upon a child nursing a mother who was taking potassium iodide.

Lewi saw pronounced symptoms of iodinization in a nursling whose mother took KI.

The uncertainly of the quantity transmitted, is an objection, and a serious one, to the use of potassium iodide as a therapy for luetic nurslings through the mother's milk.

Whether the potassium iodide leads to the rapid drying up of the milk, as Stumpf and

others claim, is not certain, but this is not an important usage, as we had occasion to remark when speaking of atropine. It is entirely unnecessary and should be absolutely abandoned.

Iodoform, even when used in very small doses, or as a dressing on wounds of vulva and vagina, will go over into the milk. The quantity excreted is quite variable and stands in no certain relation to the dose employed.

Arsenic. Brouardel and Pouchet used 12 gtts. of Fowler's solution in a nursing woman, and with gradually increasing doses could always demonstrate the drug in the milk. In one case where the patient took 12 gtts. of Fowler's solution for six days, there was 0.001 grams as in 100 grams milk.

Since 0.15 grams As. may kill an adult of 70 kg. weight, a fatal dose for a child of 3 to 4 kg. weight might be 0.006 or 0.0085 grams.

Jegourel also gives confirmatory experiments.

Therefore, arsenic ought to be used with great caution where a mother nurses her babe.

Iron. Thiemich says that it is yet undemonstrated whether or not the iron constituents of the milk are affected by the administration of this drug to the nursing mother. With an abnormally low hæmaglobin of blood under pathological conditions, there has been no investigation of the change that might occur under the use of iron. Some authors maintain that the quantity of milk is increased by iron, and Bristow claim that it improves the condition of anæmic children to give iron to the mother, and this is easily probable through the improved quantity and quality of the milk.

Lead, Zinc, Copper, and Bismuth, have received some attention, and it seems probable that both copper and bismuth pass over in insignificant quantities. Deneufenburg has established the transmission of lead in the milk. Bouquet quotes an interesting case where a woman worked in lead constantly during sixteen lactation periods, and each of the babies died in convulsions during these periods, except the last. With this child health began to fail until babe was removed from breast, whereupon recovery ensued.

Thyroid Substance has been investigated by Mossé, Cathala, Toulouse, and Cadet de Gassicourt. In one case, a three months' child with goiter nursed a goitrous mother, and the mother was given thyroid tablets corresponding to 1.5 grams of fresh gland daily. After one and a half months' treatment a slight reduction occurred in the size of the maternal goiter and a marked improvement in that of the babe. Four months from the beginning of treatment the child's goiter had disappeared entirely.

A second case has been reported from Christiana, by Ivar Bang, which shows an interesting difference: The mother took iodothyrin for three weeks, 0.3 mg. to 0.7 mg. daily. Within twenty-one days there occurred, at first rapidly, and later more slowly, a reduction quite marked in the maternal goiter, and still more pronounced in that of the child, which was two months old when treatment was begun. Diminution showed earlier and to the greatest degree in the child. The use of a minimal quantity in the early days brought about a strongly marked effect and suggests the possibility of an accidental condition as a factor in place of the iodothyrin. Experience does not as yet allow us to state whether such a diminution in a goiter could occur spontaneously.

Acids. Landerer has found that acetic acid passes over in the milk, and this may be the basis for the long current belief that nursing women should not eat acidulous fruits. While we know the stomach normally contains a large amount of hydrochloric acid, which apparently never affects the milk, it does not seem probable that the small amount that could be ingested with an acid fruit could injuriously affect the milk. The writer has continuously given every variety of fruit to nursing women without effect upon the milk. Nevertheless, the possibility of an idiosyncrasy must be kept ever in mind.

Evacuants are the subject of much discussion and many diverse opinions: Thus, Cazeaux claims that rhubarb passes over in the milk, and Schaünstein and Späth definitely maintain a contrary opinion. Most observers agree that senna goes over to the child and produces colic, as well as laxative conditions, and in addition gives its characteristic odor to the milk.

Dolan states that castor oil lends a characteristic odor and taste to the milk, and produces colic in the nursling.

Balsam of Copaiba passes over in the milk, according to Lewin; and pilocarpine, through its effect upon the skin, transiently diminishes the quantity of the milk, although not as yet demonstrated chemically.

Cascara, Aloes, and some other common drugs seem to have furnished no literature as yet with reference to lactation.

This completes the survey so far as it pertains to relatively common drugs. This investigation has been a source of considerable surprise, and of great interest and information to the writer. The scarcity of clinical reports in the literature has been no less surprising than the almost valueless character of those found. As a result, it seems probable that alcohol, opium, chloroform, ether, thyroid extract, and lead should be used with extreme care in a nursing woman until their status is determined; that zinc apparently has no effect, while all the others investigated, namely, atropine, arsenic, antipyrine, acetic acid, bismuth, brom. potassium, balsam of copaiba, castor oil, copper, chloral, iodine and its compounds, mercury, phenacetine, quinine, salicylic acid, and senna, pass over in small quantities, but very rarely to a degree injurious to a sensitive babe. Hence, it is practically never necessary to remove the child from the breast on account of drugs administered to the mother. That this position is correct is substantiated by the fact that cases occur so rarely in the literature. A logical inference from the above data may also be drawn that, owing to the total absence of transmission, or owing to the impossibility of satisfactorily measuring the amount transmitted, it is never desirable to attempt to medicate the child by means of drugs administered to the mother.

Part II

There are quite a number of anatomic and physiologic as well as social conditions that prevent the nursing of the babe. Before taking up the various organic and functional diseases which may interfere with the mammary function, it is necessary to refer briefly to a few anatomic peculiarities which quite definitely and obviously exclude the child from the breast. For instance, we may mention inverted nipple, a relatively common maternal anomaly, while, on the part of the child, one may find a facial paralysis due to forceps, or to the difficult passage of the inlet in a case of contracted pelvis. Here, too, must be considered the hare-lip, which requires artificial nourishment, since suction is impossible with a labio-palatine fissure.

The condition widely known as "tongue tie" was for years thought to interfere so positively with nursing that the division of the frenum was quite common, but we now know that interference with nursing from such a cause is not possible. It was Petit, I believe, who first showed that the division of the frenum was useless and sometimes dangerous, while Escherich has demonstrated that in the act of nursing the babe does not draw back the tongue, but compresses it between the nipple and the inferior maxillary bone.

Some of the more evident conditions that require the removal of the child from the breast are to be found in the nipple, as a direct consequence of the nursing process. The child's mouth under such circumstances is by no means harmless, since it furnishes a continual rubbing, grinding, and pulling effect by the action of the gums and tongue; a softening or maceration by means of the saliva and milk; and supplies with the saliva multiple forms of infective agents to supplement those already present upon or about the nipple. On account of the germ contents, the saliva itself is always pyogenic when injected under the skin, and easily produces an abscess.

Owing to the mechanical effect produced by the babe's mouth, therefore, in nursing, injuries occur varying from the blister, excoriation, erosion, fissure, and deep crack of the nipple proper, to the semicircular and circumscribing break in the tissues at the junction of the nipple with the breast beneath.

The occurrence and character of an injury depend upon many things. More occur upon badly formed nipples than upon the well-formed, as one would reasonably expect. Thus, conical nipples are injured in 56 per cent of the cases, while the cylindrical shape furnishes only 47 per cent. Then, too, the more powerful the child, the greater the liability of injury. Primiparæ are affected oftener (56 per cent), as compared with multiparæ (47 per cent), in

whom the nipple has been worked into shape by previous nursing and the wrinkles in the nipple skin either stretched out or obliterated. These wrinkles in the nipple are important because they are the invariable starting-point of fissures. The skin is much thinner and more tender at the fold, and being less adapted to resist mechanical insult, it cracks in the act of nursing, the crack always corresponding to the course of the fold. On the whole, Platzner's statement must be accepted that nearly one-half of all nipples show wounds or breaks of some kind. These injuries in turn are all openings through which infection may enter the breast and cause serious disturbances, but even in the beginning when very painful, or if the patient is particularly sensitive or nervous, the removal of the child may be required.

Usually, by means of local applications or the interposition of the rubber nipple-shield for a few days, these disorders, which rarely occur after the first ten days, can be readily cured. If not, the babe must be taken away and the nipple put at rest until relief is secured. If not cured quickly, danger to the breast threatens, infection may take place and extend into the deeper portions of the gland. The condition would then be more serious and the child would have to be removed on account of the general glandular involvement. Whether the infection is parenchymatous, acinous, or subglandular, the breast must be put at rest by the removal of the babe, and appropriate measures undertaken. The formation of an abscess is quite probable; and both the breast and the child are better for the separation. In some instances, when the duct has carried the infection to the deeper convolutions and pus forms, the child may nurse almost pure pus for days before the discovery is made. It is apparent that inflammation of the ducts is most frequent among women who have but little milk, since babes must nurse so often and so long that they thereby produce excoriations, which subsequently become infected.

Winckler and Koehler estimate that six per cent of lying-in women are thus affected. Theoretically, this infection may occur in three ways, either by the blood, the lymphatics, or the lacteal ducts.

It has been demonstrated that the micro-organisms of mumps (if such there be), of typhoid, and of puerperal fever may by metastasis produce mammary abscess — the germs being transported in the blood. Escherich has also shown by animal experiments that microorganisms in the blood can be eliminated from the breasts by means of the milk, as they are from the kidneys by means of the urine. This will be referred to again when systemic diseases are considered. Infection by the lymphatics is extremely rare, and even denied by competent authorities.

Contrary to what one observes in ordinary lymphangitis, the ganglia of the axillæ often remain untouched in glandular infections and inflammation of the breasts. The propagation of the infection takes an inverse direction to the course of the lymph which is absolutely contrary to the usual march of a lymphangitis. Besides, the infection is often localized clearly in one lobe of the gland; and how can one explain this phenomenon if one admits infection by the lymphatics, since the mammary lymphatics quite freely intercommunicate?

Where the infection enters the connective tissue, it may well spread through the lymph channels. Infection by the lacteal ducts, on the contrary, explains quite easily the lobar form of infection and the succession of many isolated abscesses, since each lobe of the gland opens by a duct at the summit of the breast.

Chassaignac, Budin, Arbel, and others have furnished irrefutable proof of the possibility of localization of the inflammation in the ducts. It has been observed by many that when the gland is pressed, pus and milk may run from the same opening. It has been demonstrated conclusively that congestion or mammary engorgement will not *per se* produce inflammation.

Delbet, by experiments on animals, and Williams and Storrs, from observations on a series of 175 cases at Johns Hopkins Hospital, have shown conclusively that milk stasis in the absence of infection is a very innocent condition.

It is quite obvious that the ingestion of pus by the nursling is a serious matter, but just how serious will be better understood by a short review of the disturbances traced to this cause in the various published reports. Thus

we may have a diphtheroid stomatitis; retropharyngeal abscess; gastrointestinal dyspepsia, light and without fever; toxic infectious dyspepsia, light or grave; cholera infantum; multiple subcutaneous deep abscesses, following intestinal intoxication and digestive trouble, with fatal result, as happened in a case seen by the writer.

Otitis media may occur by propagation through the Eustachian tubes, and finally the toxins may be absorbed without intestinal disorder, and produce a genuine septicæmia. By external contact the same pus may produce in the babe submaxillary abscess; multiple superficial subcutaneous abscesses; catarrhal and purulent conjunctivitis or ecthyma, either by direct cutaneous contact or by skin elimination after the intoxication following digestive trouble. Also may be mentioned thrush and bronchopneumonia, which may develop sequential to the gastrointestinal disturbance. Other local conditions, such as scirrhus and Paget's disease of the nipple, the latter not only for itself, but because of its constant association with scirrhus, should exclude the child from the breast.

Primary tuberculosis of the breast, as shown by Davis and others, prohibits absolutely the nursing of the babe, since it is completely demonstrated, by cases and by experiments to which we shall refer later, that the bacilli go over in the milk. The anomalies of the milk constitute a large percentage of the cases that demand the removal of the child. Aside from very evident conditions, such as insufficient quantity and another condition somewhat harder to diagnose — deficiency in quality — there are many things that will affect the milk and render it unfit for, or even fatal, to the child.

Anæmia in any form may demand the removal of the child, not alone because the condition of the blood results in an inadequate supply and a deficient quality of the milk for the babe, but also because the mother needs all her strength and power of recuperation.

Great loss of blood during labor is not *per se* a reason for removing the child, but if the woman does not quickly rally, or if she is exsanguinated, there should be no hesitation.

Saunders reports five cases in which he showed that the milk was toxic from the presence of the menstrual flow in one case, and from undetermined causes in the rest, and that its ingestion produced frequent, watery, acrid, and green stools with much colic.

According to several authorities, the *return of the periods* after delivery exercises *at times* an incontestable influence over the lacteal secretion, and therefore upon the nursling. This is a subject of great and general interest.

Tarnier states that when the periods return during lactation, it sometimes happens that the milk is diminished in quantity, or even disappears entirely for a day or so, only to return as quietly as it disappeared; at the same time, the proportion of solids increases to a notable degree, especially the butter casein and salts, while the sugar diminishes. These modifications are especially apparent while the flow is on, but may be observed many times in less degree during the intervals. Often during the flow the child digests less perfectly, cries more, becomes slightly pale and somewhat weakly. For Tarnier, therefore, the return of the periods is an unfortunate event in the course of lactation, but he does not on that account require the child to be fed artificially.

Budin also finds that when the flow returns during lactation there is a loss of weight in about one-half the cases, diarrhœal conditions, vomiting, erythema of the breech, etc. The importance of this will be recognized when it is stated that nearly half (41 per cent, Roche; 43 per cent, Du Boissard; 58.7 per cent, Heil) the women have a return of the flow during lactation. It simply means that the child should be carefully watched (weighed), and if in certain cases one notes green diarrhœa, irritating to the skin, and a diminution of weight at the menstrual epoch, a change should be considered, but not if the child continues to gain.

The sequential injury is far more likely to fall upon the mother, owing to the additional drain upon the system, and it might be necessary on *her* account to remove the child.

New pregnancies sometimes occur during lactation, and it is generally recognized that ovulation continues whether the menses return or not; hence a voluntary prolongation of lactation to avoid conception is futile. It may be added, however, that the more regular the

periods, the more liable is the woman to conceive.

Poirier has observed the effect in a number of cases where conception occurred during lactation. In 80 per cent, no perceptible harm was produced upon the babe, but in 20 per cent, immediate withdrawal from the breast was required. The pathological conditions that demanded the removal, were not found, although possibly reported. This, then, is another case for watchful expectancy and the removal of the babe, if reasons therefor should arise. One should also keep in mind the possibility that the mother may not be able, especially in large cities, to sustain simultaneously a lactation and a new pregnancy. Examination of the milk at four and one-half months (Budin) shows no abnormality, but if it diminishes in quantity, of course supplementary nourishment should be given.

There is another series of cases where quantity or quality of the milk is interfered with by the occurrence of certain emotions, such as *fright, anxiety*, or even *excessive joy*. A woman in Tarnier's clinic, greatly frightened by a burn, lost a large part of her milk, and the child had vomiting and diarrhœa, which did not cease for three or four days.

Vomiting, diarrhœa, loss of weight, and sometimes erythyma, are the characteristic symptoms under such conditions.

Long-continued domestic disturbance or anxiety may produce the same result.

A wet-nurse may become homesick, and in consequence the milk dries up. Very sensitive women during lactation may become so wrought and nervous that the composition of the milk is injuriously modified and the health of the babe affected. This has been ingeniously explained by Spolverini, who found an oxidizing agent constantly present in the milk of normal women; and under the influence of profound emotion, great danger, or extreme grief, the conditions favorable to its activity are provided. When the milk is examined microscopically after such nerve-storms, there is found an unusual number of colostrum corpuscles and leucocytes. The deduction is, that for certain reasons, the milk tends to return to the colostral state. The oxidizing ferment is always present, and when its activity begins, colostrum is the result; and knowing that colostrum is ordinarily a laxative, it is not difficult to see that in large quantities it may produce many and varied disorders in a child. If the Czerny theory is correct that the colostrum corpuscles *are* leucocytes, then the field for speculation and investigation is greatly broadened.

The Spolverini theory, however, does not satisfy all the conditions, nor throw much light on the nervous and convulsive disorders, nor on the meningeal phenomena. It is recognized to-day from the work of Gauthier that alkaloids, analogous to ptomaines, may be produced in living organisms by the malassimilation of albuminoids. Anger, or any violent emotion, aids this malassimilation and surcharges the organism with injurious products, which may pass into the milk and provoke a true toxæmia in the nursling — an eclampsia analogous to that of pregnancy. When the emotional disturbance occurs but once in a woman, the child may be left at the breast, but when the condition is a habit, or occurs frequently, the child should be removed. In cases of mania and epilepsy, it may be unsafe to leave the child with the mother; and while it is rare for an epileptic attack to occur without enough warning to enable the mother to put the child in a safe place, it is better to feed such a babe artificially.

Albuminuria has been the subject of a great number of discussions from the standpoint of the babe and the mother. Gamulin and Jockewitch, between the years 1896 and 1899, collected over 200 cases of albuminuria in women who nursed their babes, and in all these cases the babes left the hospital in good condidition and gaining. Dumas believed that lactation was not materially influenced. In general, then, it may be said that albuminuria persisting after delivery is not incompatible with giving the child the breast, from the babe's standpoint; but it is certain that an albuminuria sufficiently prolonged to affect the maternal health seriously should, on that account if no other, require the removal of the child. There are certain cases, besides, where the mother has a grave nephritis, with œdema and uræmic symptoms, that the milk is presumably charged with toxins and does not have good nourishing

qualities, and in these cases the nursing of the child should be forbidden, in its own behalf.

Osteomalacia, a disease accompanied, if not caused by, malnutrition, imperatively prohibits the child from nursing, on account of the mother.

Cardiac lesions. Demelin states that the "nature of the affection matters less than its degree of gravity and its effect upon the general health." Thus, no one would advise the nursing of a babe when the mother has an asystolic lesion; while, on the contrary, many women with pure mitral regurgitation may and do nurse their babes without injury. All depends on the state of the general health and the manner in which the heart bears the strain during pregnancy and labor. If in the course of the pregnancy there has been no marked functional inconvenience, no persistent albuminuria, no hæmorrhages of various kinds; if, during labor, no pronounced arryhthmia has occurred, nor serious hæmorrhages, the woman may nurse her babe under careful medical supervision. If, on the contrary, the cardiac lesion is not well compensated, lactation ought to be discontinued as a source of fatigue and prostration. It is not desirable, at the present time, to go deeper into the relationship between lactation and various organic and functional diseases. The interesting group of bacterial conditions demands attention.

Before taking up the various local and systemic infections, it will be well to review some of of the pathological developments of the last decade. For many years it was maintained that the microorganisms could not pass over in the milk, and even at the present time there is much ignorance on this subject. It is very important to this paper to determine whether or not germs do go over in the milk, and what effect, if any, such a transmission could produce.

From the freedom and ease with which the bacillus coli traverses the tissues and migrates in the body from one organ or cavity to another, one would expect to find this germ present occasionally in the breast milk, but I have been unable to find such a case in the literature.

Longard found staphylococci in the milk of febrile puerperæ, with which he inoculated lactating women, and in a few hours found the germ in the breast milk.

Michael and Merritt have worked along the same lines with positive results.

In his researches upon animals and upon women having puerperal fever, Rossi Doria concluded that the germs passed intact through the mammary filter, and in great part were eliminated by the lacteal secretion.

Karlinski's experiments are quite to the point: A woman had puerperal fever and an erysipelas. On the fourth day of the febrile state, before any sign of the erysipelas had appeared, five c.c. of milk was drawn from the left breast, under antiseptic precautions. The examinations revealed the presence of staphylococcus pyogenes (aureus, albus, and citreus), staphylococcus cereus (albus and flavus). The child was removed from the breast, and on the fourth day exhibited a parotiditis and catarrh of the intestines, which ended fatally on the tenth day. In the blood and the intestinal contents were found the same organisms as in the maternal milk

After this clinical observation, Karlinski undertook a series of experiments to establish, if possible, whether among animals subjected to inoculation of staphylococcus pyogenes, one would find these in the milk, and if the progeny would be infected by such milk.

Without quoting the experiments in full, it may be stated that not only were the germs found in the maternal milk twenty-four hours after the inoculation and for eight days thereafter, but the young nursing rabbits that died on the sixth, eighth, and tenth days revealed the germ throughout the alimentary canal and in the spleen. One also exhibited abscesses of the mouth and spleen, in which the germ was found.

These experiments were repeated many times, and Karlinski concludes: That the passage of pyogenic microorganisms from the blood into the milk ducts is possible; that the passage occurs in a relatively short time; that the microorganisms that appear in the uterus during the involution may also appear in the milk; that animals nursed by infected mothers die in relatively large numbers from pyosepticæmia.

It is most curious, in this connection, to find that the milk of sound women also contains many pyogenic bacteria (staphylococcus albus).

Cohn and Newmann found microorganisms in 38 out of 41 cases, most often the staphylococcus albus, which seemed to abound in the peripheral portions of the lacteal ducts; and the authors show that they came from without; that they do not alter the milk; that they are not injurious to the nursling; and that they do not produce any inflammation in the mammary gland.

Honigman found only four out of 76 normal women examined whose milk was sterile.

Falske and Köstlein give similar reports.

It seems probable that these germs become actively injurious only in the presence of a break in the mucous ramparts, so far as concerns the mother; but the milk containing these germs cannot be inoffensive to the nursling, though the morbid effects may be variable. This variability in virulence would depend upon special causes which might modify its action, or paralyze it absolutely. Mixed infections may also occur and make the picture atypical.

Perret reports a series of cases of infection, including tonsillitis, pelvic infections, sapræmias, lymphangitis, and icterus, wherein the nursling remained at the breast, and from which he concludes that this method might be followed possibly in a majority of the cases.

In the light of various experiments on animals and knowledge acquired from other sources, this would seem to be extremely hazardous. The infectious and contagious diseases, such as typhoid, all the eruptive fevers, erysipelas, acute pulmonary affections (like pleurisy and pneumonia), and acute articular rheumatism, are direct and positive contraindications to nursing the babe — dangerous to the mother on account of exhaustion, and dangerous for the child on account of the possibility of germ transmission through the milk, and because the milk is altered in character and loaded with toxic material.

Brieger and Ehrlich particularly have shown the great facility with which diphtheria toxins may pass over in the milk.

Tuberculosis is probably the most important, because one of the most common, of the contraindications. Systemic tuberculosis exhausts the mother, makes the milk deficient in quantity and quality, and loads it with toxins. Moreover, the close contact of mother and child will almost invariably infect the child without the intervention of the milk. The same is true of old pleurisies that may possibly be tubercular. Where the woman has had a coxalgia, but has been cured for many years, and is in sound health, the babe may nurse.

Furthermore, the question of the transmission of the germs through the milk has been definitely settled, not only by the demonstration of the passage of staphylococci, but by the transmission of the bacillus tuberculosis itself.

Hippolyte Martin has shown that tubercular milk will infect an inoculated animal.

Koubossoff has shown that animals inoculated with tubercular pus will exhibit the bacillus in the milk in about two weeks.

Despite Koch's statement, it is well known that tubercular cattle will produce tuberculosis through the milk. Nonewitsch (quoted by Cornet) reports 3 cases where the bacillus of tuberculosis was carried over in the milk, and in one of these cases the woman presented only very slight evidence of pulmonary infection.

Roger and Garnier report a woman, with pharyngeal and pulmonary tuberculosis, who died seventeen days after labor. The bacillus was established in the milk without clinical evidence of tubercular lesions in the mammary gland. The milk, aseptically obtained, was injected under the skin of a rabbit, which died thirty-three days later with typical lesions of general tuberculosis. The child, nursed by the mother for two days only, died six months after birth. The post-mortem shows granulations in mesentery, liver, spleen, and kidney. The localization of the lesions showed that the bacilli were absorbed through the alimentary canal.

A question at once arises in regard to the defensive power of the gastric juices; and, fortunately, observations are reported which bear on this subject.

The experiments of Chauvean, Weiser, Bollinger, Hirshberger, Falk, Baumgartner, and others, go to show that the digestive juices have not sufficient power to interfere and protect the system.

Strauss and Wurtz made experiments with pure culture of tubercle bacilli and pure digestive juices, and were enabled to infect animals with the mixture after exposure to the digestive

action for six hours — a much longer time than the bacilli ordinarily would remain in the stomach. The bacillus may also be transported by the leucocytes in the mesenteric ganglia through sound epithelia. Jemma also gives confirmatory experiments. It is, therefore, quite well established that tubercle bacilli can and do pass over in the milk of the mother, and can and do infect the child through the alimentary canal, despite any possible interference from the gastric juice.

Basch and Weleminski were unable to demonstrate the bacillus in the milk, but freely admitted the injurious effect produced by a tubercular mother on the babe at the breast — an effect which they concluded was due entirely to the transmission of the toxins of the disease.

Pneumonia. The pneumococcus has been recognized and identified in the blood by many authors. Foa and Bordoni-Ufreduzzi found the germ in the uterine vessels, in the placenta, in the maternal liver, and in the blood of the fœtus.

Aymard inoculated a lactating rabbit with a culture of pneumococcus. Six days later the rabbit died from a generalized infection, and with a considerable quantity of diplococcus in the milk. Two hours afterward the young also died, and numerous diplococci were found in the blood, both by microscopic examination and by culture.

The microscopic examination of the breasts showed numerous diplococci in the ducts and no inflammatory reaction in the adjacent tissues.

Aymard also reports a case: The woman was seized with the initial chill of pneumonia four days after labor; child removed from the breast thirty-six hours later. Mother died in three days, and the post-mortem confirmed the diagnosis completely. Babe continued well for two days after maternal death; then the milk of the wet-nurse seemed to disagree and the babe had vomiting, green stools, diarrhœa, fever, and tympany; autopsy showed a suppurative cerebrospinal meningitis, and purulent peritonitis. The pneumococcus was the only organism found.

The one deduction possible is, thatthe woman with pneumonia should not nurse her child. Not alone pneumonia, but every form of germ infection should prohibit the child from taking the breast, since the bacteriæmia is quite liable to infect the milk. This includes typhoid and cholera.

Syphilis is so well studied that it is sufficient only to make mention of it here.

Acute Contagious Diseases and Erysipelas in the mother would, of course, preclude the nursing of the babe.

From a study of the foregoing material, it seems certain that the mother, who is abandoned to the care and nursing of her babe without expert advice, quite commonly gets bad results and not infrequently fatalities. The increase in the weight of the babe should be regularly followed on the scales and accurately recorded; the stools frequently examined; and when cutaneous manifestations appear, such as erythyma, even though slight, the milk of the mother should be analyzed and her physical condition investigated.

We find that while the return of the menses need not necessarily contraindicate nursing the babe, yet if the child begins to show digestive disturbance and intestinal irritation, this source of trouble, so often overlooked, should be taken into consideration, and its effect carefully estimated.

The same deduction must be made in regard to mental disturbance during lactation, while albuminuria and cardiac lesions, unless quite unimportant, will more often require removal of the child on account of the mother rather than the child. Removal from the breast is imperatively demanded for the sake of mother or child, or both, when lactation is complicated with mania, or epilepsy, except in very light and infrequent attacks.

Salge maintains that a mother with scarlatina, measles, and diphtheria should continue to nurse her babe, and claims that the ingestion of such a nourishment will render the babe forever immune to these diseases.

Considering the ease with which streptococci enter into and disseminate themselves through the blood, this theory has very serious objections, and would appear to be highly dangerous. The risk in nursing a child under such conditions is far more hazardous than any possible benefit that could be secured. Further, it is

not probable that immunity to diphtheria would be conferred for a longer period in any case than when secured by injection of antitoxin — say from six to eight weeks.

Nephritis, serious and obstinate erosion and fissure of nipple, Paget's disease, scirrhus, sarcoma, or abscess of the breast, together with local or systemic tuberculosis, syphilis, erysipelas, acute articular rheumatism, acute pulmonary or pleuritic affections, and all infectious and contagious diseases, osteomalacia, and all puerperal infections that are not mild and transitory in character, as a rule, demand the absolute separation of mother and child.

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SUCCESSFUL TREATMENT OF CHRONIC GONORRHŒAL ENDOMETRITIS[1]

BY G. KOLISCHER, M. D., CHICAGO

CHRONIC gonorrhœal endometritis represents one of the most difficult propositions with which the gynæcologist has to deal. The great number of methods that are suggested for curing this condition prove convincingly how far from uniformly successful all these methods are. I, for one, have to admit that, after having tried for many years everything that was recommended in this line, I failed in the vast majority of cases to obtain the desired result.

In recent years, however, in the light of certain urological experiences, I have developed a method which, I think I am entitled to say, answers the demands. There are two main desideria that a method for treatment of gonorrhœal endometritis must meet: 1. It must eradicate the gonorrhœal infection; and, 2. It must stop the abnormal discharge. Furthermore, the intrauterine applications should neither provoke spreading of the gonorrhœal infection to the appendages, nor make old processes there flare up again.

Before entering into the details of this treatment, I wish to state that the method was employed in 43 cases of this kind; of these, 4 discontinued the treatment before it was finished, 7 could not be reached after having been discharged as cured, 3 became reinfected; the balance appear to have been cured, the last case related in this article having remained well through four periods of menstruation.

[1] Read before the joint meeting of the Chicago Gynæcological and Medical Societies, March 25, 1908.

A drug, in order to be employed successfully in the cervical canal and in the uterine cavity, should be a strong germicide and should produce sufficient reaction to secure an intense shedding of the integument; at the same time it should not produce cauterization to such an extent that a real eschar be produced. It is a matter of experience that in chronic gonorrhœal mucosa troubles only frequent applications, repeated at short intervals, will produce results. If the drug applied should cauterize the mucosa, the next application would touch only the crust and not reach the tissue in which we wish to produce reaction. As such a drug, albargin recommended itself, having shown all these qualities when used in gonorrhœal troubles of the urethral mucous membranes.

In detail the treatment may be described as follows: At first every uterus, in which intrauterine applications are contemplated, should be tested as to its reaction toward the introduction of a foreign body into its cavity. If this precaution is omitted, the operator will occasionally encounter a uterus that will react to any intrauterine interference with such violent contractions that the resulting uterine colic may last several hours, even causing the patient to faint from pain. Such a test is made by introducing, very slowly and carefully, into the uterine cavity a uterine sound lubricated with glycerine, finally touching the endometrium of the top with the tip of the sound. Under ordinary circumstances the patient will not display any signs of discomfort, but if one encounters a highly sensitive uterus, the sounding of the endometrium at the fundus will be perceived as a rather keen pain, and the uterine musculature will react by contractions, engaging the sound. In such event, these soundings should be repeated until the sensitiveness of the uterus is sufficiently obtunded to allow of actual treatments.

In administering the treatment, the cervix is opened by a bivalve speculum, after which the portio and the adjacent parts of the vagina are cleansed, either by irrigating or by swabbing with an antiseptic solution. Then the uterine cavity and the cervical canal are freed from adherent pus and mucus, by introducing a swab saturated with a concentrated solution of bicarbonas sodæ. These swabs are best made of toothpicks armed with absorbent cotton; they are, of course, sterilized before being used, and are inserted into the uterus by means of a long, slender forceps. It is absolutely unnecessary, and in fact rather objectionable, to try to wipe off the mucus with this swab. If this applicator is left for a minute or so in the uterus and then withdrawn, the liquified mucus and pus is expelled as a big plug; this is now removed and the uterus is ready for the introduction of the silver solution. In case difficulties are encountered in introducing the swab, the anterior lip of the portio is seized by a tenaculum or by a vulsellum; the uterus, however, should never be drawn down, it should simply be steadied, otherwise, in snapping back after being released, it might pump infectious matter up into the parametria and the appendages.

A swab soaked in a three per cent albargin solution is now introduced into the uterine cavity, and is allowed to remain there for five minutes; after it is withdrawn, a tampon, carrying an ichthyol-glycerine solution of equal parts, is inserted into the vagina and pushed up against the portio. The next morning, the patient removes the tampon, cleanses the parts with a hot douche with salt water, and is ready again for the next treatment. As a rule, a slightly bloody discharge appears after a few treatments. This interferes not at all with the continuance of the treatment; on the contrary, it seems rather a favorable symptom.

These treatments are continued daily until the discharge disappears and until repeated examinations of the mucus from the uterus and from the cervix fail to show either pus cells or gonococci. These specimens should be taken after provocatory influences have been brought into play, such as during and after menstruation, twenty-four hours after the patient has taken some charged alcoholic beverages, or twenty-four hours after the mucosa has been touched up with a 1-1000 bichloride solution.

Occasionally one will encounter rather torpid cases, in which even the albargin solution fails to produce sufficient reaction. In such event, the necessary stimulation is brought about by saturating the applicator with a forty per

cent formalin solution and introducing it into the uterine cavity. The withdrawal of this formalin swab, however, should be immediately followed by the insertion of a large ichthyol tampon, in order to keep the formalin solution from flowing down into the vagina and vulva, an accident which excites very severe smarting on those parts. This application is occasionally repeated during the course of the silver treatment.

In case the endometrium proves to be very rough, the whole course of treatment is preceded by an abrasion of the mucosa with a sharp curette; this is done, not because it is believed that such interference influences the gonorrhœal infection, but in order to put the tissue in a condition more favorable for treatment. The applications are started after the discharge following the curettement has lost its bloody character.

A success can, of course, be expected from this treatment only if one succeeds in excluding the source of reinfection. The time during which the diseased cervix and uterus are treated is also the best time for attending the chronic gonorrhœa of the male partner.

CASE REPORT OF PLASTIC REPAIR IN OLD GUNSHOT WOUND OF THE ABDOMEN

By J. K. MARDEN, Marsovan, Turkey

Greek patient from Ordu, age 35.

Previous history: Was shot with a rifle-ball six months before. The ball passed through the ileum, coming out of the abdomen two inches above crest of the pelvis in the left hypogastric region. There was no surgeon in his village, and nature healed the wound as best she could. The man lived, but there was a fecal fistula.

He went to Trebizond where a surgeon attempted to close the wound. The result of this operation was a large slough, and the man was much worse than before. Six months after the accident he came to our hospital, two days' journey by steamer on the Black Sea, then two days' by wagon. Examination of patient on arrival, showed a large opening in the intestine, through which all fecal matter discharged. There was much cicatricial tissue about the wound — the result of the unsuccessful operation done some three months before.

Operation: Under chloroform, the wound was cleansed as thoroughly as possible. The intestines had been emptied as completely as possible the day previous, and a small tampon of sterilized cotton, attached to a silk thread, was introduced into either end of the intestine. The field of operation was then thoroughly cleansed with alcohol, ether and bichloride. An incision was made in the normal tissue, on three sides, down to the peritoneum. Next to the pelvis, the parts were very adherent. The peritoneum was opened, and the intestine drawn out as far as possible. The abdominal cavity was carefully packed-off with sterilized pads. Though the wound was over the descending colon, the small intestine was the one that was injured. This was resected, an end-to-end anastomosis was performed, and the intestine returned.

The difficulty lay in closing the abdominal opening. So much had sloughed off or was mere cicatricial tissue with no muscular tissue beneath, that I decided to take a flap from the groin, including skin, muscle and fascia, and do a plastic operation. As far as possible, the peritoneum was approximated, after which the new flap was adjusted.

The patient made an uneventful recovery and went home in one month, perfectly well.

DEPARTMENT OF TECHNIQUE

A SIMPLIFIED TECHNIQUE FOR SUPRAVAGINAL HYSTERECTOMY

By WILLARD BARTLETT, A. M., M. D.
Professor of Experimental Surgery, Washington University, St. Louis

Most supravaginal amputations of the uterus, done within the abdomen, are for myomata of that organ. We know perfectly well that a characteristic heart lesion complicates many of these cases; hence, it is of paramount importance that such patients be operated upon with the greatest celerity which may be consistent with first-class work. Of course, the anatomical variation in these cases is so great that it is impossible to consider them as a unit; still, for the sake of argument, I will state that considerable observation in this line of surgery has convinced me that slow operators, in general, consume on an average something like two hours in an operation of this kind; fairly good operators take something like one hour for it; whereas, it seems to me that our really expert surgeons, who are doing a large amount of work, usually take about thirty minutes to complete the procedure, when the classical ligations, etc., are made, and the abdomen closed in layers.

A simple method of treating the stump, which we have been using at St. Anthony's Hospital for the past year or two, facilitates the work so greatly that practically all of these operations are done, without any haste, within a period of fifteen to twenty minutes, and I am sure that some of the wizards of the profession could complete the whole thing in considerably less than the time specified.

The round ligaments are clamped, and divided near their origin; the bladder stripped down as far as may be desired on the anterior surface of the cervix; the broad ligaments are clamped and cut close to the uterus, and then a ligature passed around the branches of the uterine vessels which run parallel on each side to the neck and body of the organ, it being drawn tight as they are cut across. These six structures being divided, nothing remains but to cut across the cervix in such a manner that a wedge-shaped opening is left, transversally disposed. Two clamps remain on either side, one on each round ligament, and one on each broad ligament. These structures from one side are placed in this wedge-shaped opening; a ligature is carried through the posterior uterine wall, and under them on out through the anterior wall, then brought back over the masses held with two clamps on the same side, and tied, as though the object were merely to close up the wedge-shaped defect. The same needle and thread then take care of the two clamped masses on the opposite side, in exactly the same way. This maneuver being complete, it will be seen that the round and broad ligaments from the two sides are simply locked in the wedge-shaped defect between anterior and posterior walls of the uterine stump. Care is taken to pass the needle through upper and lower edges of these flat pedicles, when passing the same under and over them, in order that there may be no possibility of them slipping out of the wedge-shaped defect. The resulting knots are on the posterior wall of the stump.

Now, to complete a peritoneal autoplasty, it is merely necessary to catch one bite with the same needle in the peritoneum covering the bladder, and tie the two resulting ends. It will be seen that but one needle and thread have been used in the entire procedure within the abdomen, exclusive of the treatment of the two uterine arteries, and the operator who completes this technique for the first time will be astonished when he looks at the clock.

We realize that it is a return from the ligature of each vessel singly to ligature "en masse," but this theoretical objection does not outweigh the value of time saved, in our opinion.

A very large number of these operations have been done at St. Anthony's since we have been using this treatment of the stump, and in only one instance have we found it necessary to vary the procedure and in that, not enough of the stump was left for this performance.

AN IMPROVED HODGEN SPLINT FOR THE TREATMENT OF FRACTURES OF THE THIGH OR OTHER PAINFUL AFFECTIONS OF THE LOWER EXTREMITY[1]

By GEORGE S. BROWN, M. D., Birmingham, Alabama

The Hodgen splint was a modification of the Smith's Anterior Splint. The Smith's Anterior consisted of a stiff wire frame, bound to the anterior aspect of the thigh and leg; and was useful chiefly for suspension. Hodgen widened the space between the side-bars of this splint so that the limb could drop between and, pinning the encircling turns of roller bandage to the side-bars of the frame, cut away that portion of these turns which covered the front and used the under-lapping portions as a hammock to support the limb. As his intention was to use this modified splint for extension as well as suspension, he adopted the adhesive strips and foot-block of Buck to fix the limb in the splint.

Although a few good articles have been written on this subject for the journals, the text-books, as a rule, have given it very little space. With the exception of the very excellent article by H. H. Mudd in Park's Surgery, they have usually printed the picture of it, saying nothing at all of the principles of its action nor of its application.

Another reason for the general neglect of this useful appliance, is the difficulty of conveying in written words an adequate idea of its usefulness, or indeed of making the application of it clear. The chief reason, however, for its being so often abandoned after one trial is that the splint, as used by Hodgen and as at present pictured in the text-books (Fig. 1.), is troublesome to understand and adjust, and difficult to keep in order. Those surgeons who have used it with my modifications though, have, I believe, continued to do so.

It is used by all the surgeons and general practitioners of our city; and at least six of these have had wide experience as internes in as many of the large hospitals in the east, and were trained to use different methods. They unite in saying they have never seen any method give such uniformly good results as they get from this.

Although I have been able to interest a number of surgeons over the country through short journal articles from time to time, during the last 10 years, and some others by actual demonstration, I hope that the more elaborate illustrations and more minute explanations of this paper will further the more general adoption which I am sure this very useful appliance deserves. It should, at least, lead to a fair trial of it when it is generally admitted that there is, at present, no treatment for fracture of the thigh that is anything like satisfactory — none at least that can be used by the surgeons, occasional surgeons and general practitioners who, in the nature of things, are compelled to treat the majority of these cases.

The principle of the Hodgen splint is to suspend the limb from the bed and to *utilize the weight of*

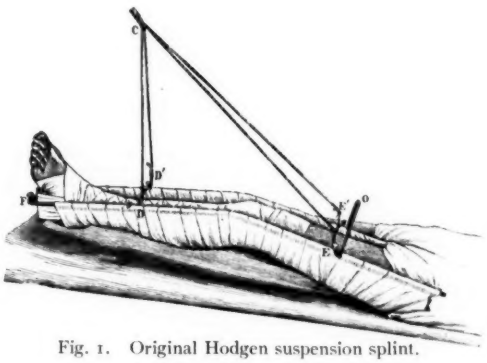

Fig. 1. Original Hodgen suspension splint.

the limb in making the extension. When the limb is suspended, the bed is rolled toward the wall at its head until the suspension-cord is at a sufficient angle, and then the foot of the bed is elevated on blocks. When this is done the cord is pulling upward and away from the body, while the weight of the limb is pulling directly downward toward the bed; the resultant pull will be between the two or away from the body (the counter-extension or fixed point) toward the foot of the bed in a horizontal direction. The amount of the pull depends upon the weight of the limb and the angle of the suspension-cord; the direction of the pull is always horizontal but always in line with the swaying suspension-cord. This principle I have found to be of easier application and adjustment with the following changes I have made in the splint:

First. As the under-lapping strips of roller bandage would continually roll up in strings and

[1] Read in part at meeting of the Southern Surgical and Gynecological Association, New Orleans, December, 1907.

cut the patient's skin and otherwise get out of order and, as they were clearly merely a survival of the roller bandage of Smith, I substituted one piece of cloth as a hammock. In order to keep this hammock taut, I fastened the four corners of it to the loops at the upper and lower corners of the frame to be described later.

Second. I substituted four leather straps for the two supporting cords. These straps had holes close together, so that by shifting the buckles the lengths could be adjusted nicely. At the suggestion of my friend, Dr. Perkins, of New Orleans, I later replaced these straps with the small cords and tent-blocks as being better and cheaper.

Third. In the endeavor to find some means of measuring the amount of the traction applied, I placed a cheap spring-scales on the end of the suspension-cord and on the hook of this hung the four small cords. I found that it answered another useful purpose in giving a very comfortable jigger or elasticity to the pull.

Lastly. While retaining the traction adhesive strips, I have done away with the foot-block of Buck. The application, adjustment and maintenance of this splint are simple; and I hope the following detailed description of it may not appear too complicated.

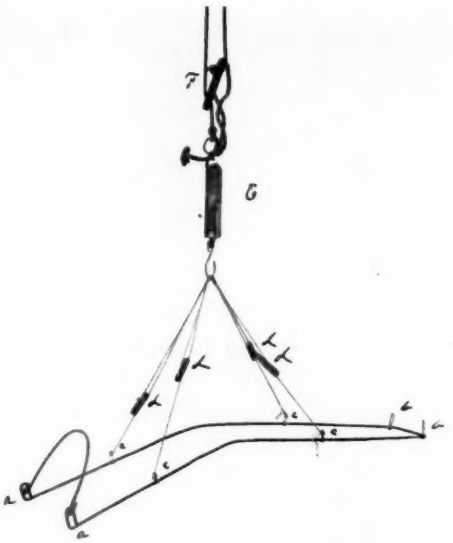

Fig. 2.

Fig. 2 shows the frame of the splint. It is made of $\frac{3}{16}$ inch iron rod or No. 4 wire, all in one piece except for the arch "J" which regulates the width of the upper end of the hammock. Making this frame "rights and lefts" by making one side longer than the other is not necessary. Some have made this frame of adjustable length, but such invariably get out of order and are more complicated and expensive by far than this simplified one that any tinner can make to measure in half an hour.

The loops, aa., at the proximal end are for the accomodation of the hook-ends of the arch "J" and also to afford a place through which to fasten the upper corners of the hammock cloth to keep it from slipping downward toward the knee. The width of the distal or foot-end of the frame is usually about four inches, and each corner is equipped with an upright loop of stiff wire (bb.) two inches high and soldered tightly to the corners. These loops afford anchorage for the lower corners of the hammock and keep it from slipping up toward the knee. They also take the place of the foot-block by affording a place through which the muslin ends of the traction-strips can be passed and tied opposite the sole of the foot. Four short wire-loops or rings are fastened to the upper aspect of the side-bars of the frame in the positions indicated by c,c,c,c. For small, medium and large sized adults the upper two of these rings will be almost always about seven inches from the level of the perineum; and the two distal ones should be about 14 to 16 inches toward the foot, from the upper ones, or about 21 or 22 inches from the perineum. With these rings in these positions it does not matter if the frame extends some distance beyond the foot. The frame should be slightly bent at the level of the knee before it is applied but it is made straight. With the four small side-rings soldered tightly in the places indicated, a stock of three frames of different lengths will cover all cases. The frame should be at least two inches longer than the limb, but will answer as well if it is four or six inches longer. The wire rings c,c,c,c, are for the attachment of the four small supporting cords, equipped with the tent-blocks d,d,d,d. These small supporting cords should be made of hard, plaited cord, $\frac{1}{8}$ inch thick, that will not stretch; but if this cannot be had the ordinary trot-line "staging" will do. The small tent-blocks should be made of hard wood, that will not split, $\frac{1}{3}$ inch thick, $\frac{3}{4}$ inch wide and two inches long, with a hole half an inch from either end. The cord is passed through this hole and knotted; then the other end of the cord is passed through the hole at the other end in the reverse direction and tied in the wire-loop "a," thus leaving a loop in the cord above the tent-block which is to hang with its fellows on the hook of the spring scales "E." This is attached to the suspension

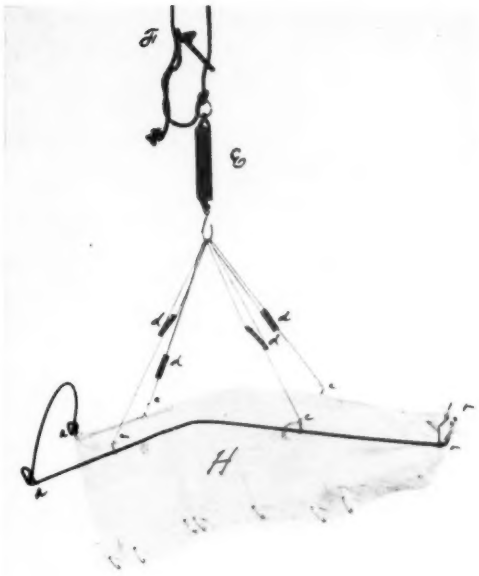

Fig. 3.

sash-cord and large tent-block "F," which are hung from the pulley-wheel in the ceiling.

Fig. 3 shows the frame with the hammock "H" attached to one side. This cloth is cut at each of the four corners and the cut corners pinned through the loops to keep the hammock taut; it is also cut two or three inches deep at the sites, c, c, c, c, for the accommodation of the small rings. It is made to fit the under surface of the limb by adjusting the safety-pins along the outer bar of the frame. The hammock is best made of light canvas or heavy unbleached muslin; and if there are wounds to be dressed, one end or the other can be unpinned and turned back out of the way while the remaining portion supports the limb. If the wound is at the knee, the cloth can be in three parts, so that while the middle one is removed or tucked back the thigh and leg portions will support the limb.

Fig. 4 shows the splint being applied to a simple fracture of the thigh anywhere from the condyles to the anatomical neck. The whole limb is cleaned and shaved and a two or three inch width of zinc oxide plaster is applied to either side, from two inches above the knee, along the leg, to two inches below the malleoli. These two inches are then folded back and stuck on itself opposite the malleoli. To each of these folded ends of the plaster traction-strips, we then pin about a foot of stout muslin roller of the same width, with two strong safety-pins. These muslin ends, k, k, are, later on, to be passed through the distal loops, b, b, and tied opposite the sole of the foot: thus fixing the limb in the splint and doing away with the foot-block. If the limb is of large size, the zinc oxide adhesive strips may be made double-thick and should always be re-enforced by a muslin roller up to the knee.

The leg having been equipped with the adhesive strips as just described, the surgeon now leans over the foot of the bed and, steadying himself against it, takes the patient's foot in one hand and his calf in the other, and straightens the limb out on a pillow. He holds it there with a gently maintained pull, just sufficient to oppose the muscular spasms of the thigh muscles. The assistant now takes the splint, to which has been attached the hammock and the four small supporting cords with their small tent-blocks, and, as the surgeon (still keeping up steady traction) gently lifts the limb an inch or two from the bed, slips it under and hooks in place the arch "J." The surgeon now allows the limb to rest upon the hammock with the pillow underneath and, while still maintaining a steadying pull with the hand which grasps the foot, with the other hand he pushes the splint well up against the perineum while the assistant passes the muslin ends of the traction-strips, "k, k," through the loops, "b, b," and ties them in a bow-knot opposite the sole of the foot, as the whole is shown in Fig. 5. (Note: unfortunately the patient used for illustration in Figs. 4 and 5, had a plaster dressing on a sprained ankle; this has nothing to do with our appliance, the Hodgen splint.)

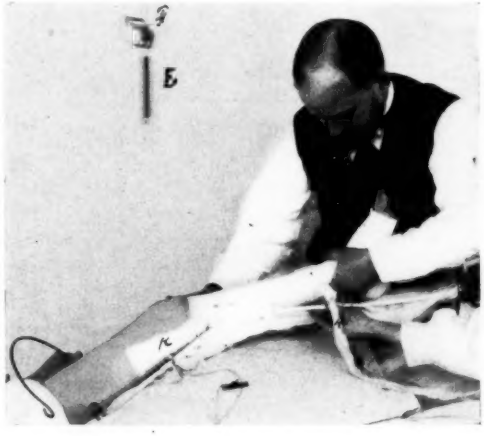

Fig. 4.

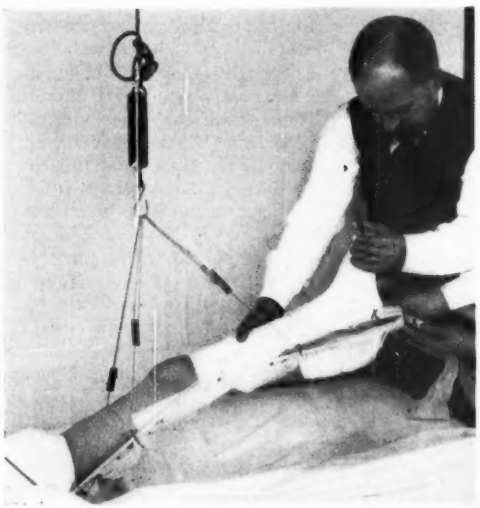

Fig. 5.

The surgeon now relinquishes the foot to the assistant and, gathering up the four small supporting cords, he loops them over the hook of the spring-scales "E" on the end of the suspension sash-cord "F", which has previously been passed over the pulley-wheel in the ceiling and equipped with the large tent-block. He then grasps the spring scales with one hand and the large tent block with the other and by gently lifting the one and pulling down the other, the limb is raised from the bed. When he releases his hold, the large tent block "pinches" and the whole remains suspended.

The safety-pins, fastening the hammock of the inner bar of the frame, having already been adjusted evenly and permanently, the pins along the outer bar are next readjusted, one at a time, to make the hammock fit the contour of the under side of the whole length of the limb. If necessary now, the the frame is again pushed up against the perineum and the traction-strips, "k, k," retied (again in a bow-knot) opposite the sole. The small cords and tent-blocks, "d,d,d,d," are next readjusted until the limb hangs level.

Next the eversion is now over-corrected by slightly shortening up the two small supporting cords, "d, d," on the outer bar of the frame, and by rotating the limb inward and propping it there with rolls of cotton between it and the outer bar, at the ankle, calf, knee and the thigh below the fracture. The bed is now moved gently on its rollers until the suspension-cord hangs exactly perpendicularly and the weight of the whole noted as indicated on the spring-scales (Fig. 6).

Traction is applied now by simply rolling the bed toward the wall at its head until the suspension sash-cord is at an angle of from twenty to thirty degress from the perpendicular. The rollers should now be removed from the foot-legs of the bed and they should be elevated on heavy blocks from 6 to 10 inches high. These blocks should have holes in the top an inch deep, for the legs of the foot of the bed to rest in, and prevent them from slipping off. These blocks should also be smooth on the bottom, so that they will slide over the floor with a slight effort to admit of easy adjustment of the position of the bed from time to time.

It will be noted now that the spring-scales indicate an increase in the weight on them; the difference between this and the weight noted when the cord was perpendicular, will be, approximately, the amount of traction being exerted. The angle of the suspension-cord and the elevation of the foot of the bed should be varied according to the weight of the patient and his limb. From a child of eight years up to a patient weighing 200 pounds, the net pull will vary from five to twelve pounds.

Long, flat, half-filled sand bags, weighing from two to six pounds, laid on the uncovered anterior aspect of the thigh will be found to serve two very useful purposes: i.e., they will add weight to the limb, thereby increasing the pull, and by their weight also they will control the muscular spasm which is otherwise troublesome during the first

Fig. 6.

week. Finally the foot of the bed should be pushed aside a few inches so that the injured limb will be pulled in a slight degree of abduction. This final position is shown in Fig. 7.

When the principles of this splint are understood and appreciated, the application of it will be found simple and the nice adjustment of it, to the many different conditions to which it is applicable substantial and efficient. This, however, was not true of the old splint of Hodgen. In the cuts of the old splint in the text-books, the swing and limb seem to hang perfectly when as a matter of fact one defect alone renders it impossible to adjust it so that it will ever hang level. This is the absence of the small tent blocks; when these blocks are not used the weight of the leg sags down and tilts forward the lower fragment of a fractured thigh, owing to the fact that *the weight of the leg from the knee down is greater than the weight of the thigh, from the knee up to the fracture*. This can be perfectly regulated in the improved splint, while in the old one these cords could not be regulated at all without letting the limb down on the pillow, and even then the "lift" could not be shifted from the leg to the thigh or vice versa. It is most important to apply this splint in a manner that will be at once simple, substantial and cheap; or rather, I mean by the latter, it can be better made if one follows the plan I have developed of making each part to be assembled such as can be made or bought for a few cents at the village hardware store. These articles are: The wire or iron frame, made by the tinner; fifteen feet of sash-cord, one and a quarter yards of light-weight canvas, one ball cotton "staging," two dozen safety-pins, one spring-scales, one screw pulley-wheel, adhesive plaster and roller bandage and the tent-blocks.

The frame should be made in the simple manner I have indicated as all adjustable screws and telescoping improvements are difficult to keep in order or, I should say, impossible to keep in order: because they will rust when not in use or will get bent and will not telescope, besides which they are unnecessary. This splint, like all other splints, should be made on the spot, though the frame can be thrown in a closet and taken out rusty ten years later, it will be found just as serviceable as it was at first, and at a small cost of time and money the other simple items can be got together new. The splint can be put up *substantially*, just as cheaply and as easily as not. Care should be taken that the adhesive strips do not stretch, that the cords will not break nor stretch, and that the tent-blocks will not split and that the cords, passing through them, will pinch and not slip.

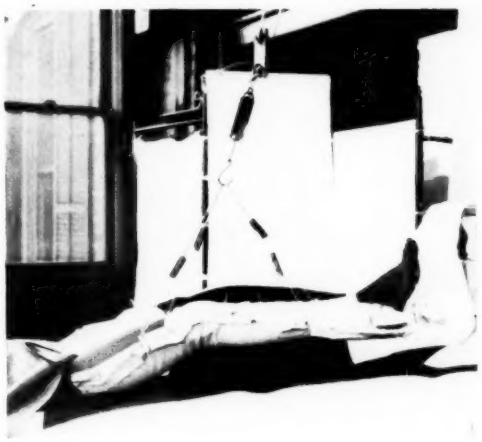

Fig. 7.

It is not necessary that the outer bar of the frame be longer than the inner, thus making the splint "rights and lefts" and allowing the upper edge of the hammock to follow the gluteal fold and support the upper fragment even in fractures of the neck, *as the fracture is always sufficiently supported by the tension of the muscles when the traction is applied*.

Figs. 8 and 9 will illustrate my meaning; a piece of broom-handle, cut through obliquely to represent an oblique fracture of the thigh with rubber bands attached to represent the muscles. The fragments overlap, when the muscles are contracted, and when the over-lapping is pulled out the muscles give a very decided splinting support to the fragments.

This is always sufficient support for simple fractures in this splint because, in addition, the muscles are, in turn, well supported by the hammock and, owing to the fact that the ligaments of the hip-joint are non-resistant, there is little or no disturbance of the seat of fracture by any ordinary movements of the patient's limb or body. *With*

Fig. 8 and 9.

every movement of the limb or body, the limb and splint swing, as a whole, between the ligaments of the hip-joint and the suspension-rope and so automatically adjusts itself, that the seat of fracture is not disturbed by any reasonable excursions the patient may make over the bed in attending to his daily wants and conveniences. What little movement occurs probably hastens the formation of the callus.

The suspension-cord should never be swung to any attachment on the bed itself, because then the angle of the pull cannot be increased nor diminished to amount to anything; neither should it be hung from a frame that is unsteady and not high enough. This splint seems to be the one surgical appliance that can be better used in a private residence than in a modern hospital, because in the former a screw pulley-wheel can be put in the ceiling. In some hospitals this will not be permitted; and in others it is not possible because the ceiling is made of concrete. In such case the surgeon's ingenuity must provide some means of suspension that will be steady and substantial and have a height above the bed of six or eight feet, though ten or twelve would be better. *The longer the suspension-rope the less the pull required.* This fact is not easy to explain unless it is that the wider range of swaying motion, with the angle of the cord much less, gives a teasing pull from side to side, that is more effective than a steady straight-away pull that is much harder—much as the rocking, swaying pull of the obstetrical forceps will effect more than the dead-straight pull. If the suspension-cord is only two feet long above the bed, and while at an angle of thirty degrees shows by the scales that there is a pull on the thigh of ten pounds, increasing the length of this cord to ten feet without altering the angle, will not increase the weight of the pull as shown by the spring-scales but will increase it in *effectiveness* fully a hundred per cent. That is, the ten pounds shown by the scales with the rope ten feet long, will give a more effective pull in the way of reducing the shortening than if the ten pounds shown when the rope is two feet long were increased to twenty, either by adding sand-bags to the weight of the limb or by rolling the bed until the angle is so acute the scales will show that much increase. With a short cord the pull must be greater to be equally effective, and the patient's movements are more restricted; he gets tired, the pull worries and hurts him; and he will finally allow himself to slip down in the bed until the foot-end of the splint rests against the foot of the bed in order to get rest from the painful pull and the cramped position. On the other hand, if the rope is long enough the patient is not made uncomfortable, hence his cooperation can always be had for the asking. Indeed, he soon learns that what the surgeon is trying to put in operation for the good of his limb is also the most comfortable. When a fracture of the thigh is put up in the manner described, with the suspension-cord at least six feet long, with the knee slightly flexed (the natural angle assumed when the limb is at rest), with the soft jigger of the spring-scales materially helping to neutralize the small shocks and jars, the patient is made absolutely comfortable and maintains the greatest pull ever needed with this splint (ten or twelve pounds) without complaint. The patient is comfortable because he has great liberty of movement, because there is no tension of the ham-string muscles and because the pull of ten or twelve pounds is so distributed it is not noticeable at any one place.

In the Buck's extension a pull of from 18 to 25 pounds is necessary, because the *friction of the limb on the bed* has first to be overcome before there is any pull on the muscles of the thigh: while in the Hodgen *the bed the limb is resting in goes along with it, and so the friction very materially lessens the pull on the traction adhesive strips instead of increasing it.* Again, in the Buck's the whole *twenty* pounds of pull falls on the straightened-out ham-string tendons while in our splint of the total pull of ten pounds at least three-fourths of it is taken off the traction-strips by the friction of the limb in the hammock.

On account of the pull of the Buck's being painful, the patient is always resorting to all kinds of shifty devices to have the pull interrupted. He will beg some friend to have mercy on him and put the weight up on a chair or the bed, where it is very common for the surgeon to find it. Or again, he will actually allow his heel to dig into the mattress until a slough takes place, rather than endure the agony of the pull. (Note: A surgical friend told me recently that he never used anything but the Buck's and that he overcame my well-founded objections to it by putting on twenty-five pounds and then for two weeks giving the patient all the morphine he wanted to keep him so easy that he would have no inclination to interfere with the pull. At the end of two weeks he puts on a heavy plaster dressing after the manner of Whitman. His results were only *fair*.)

When, with his pain-stimulated endeavor, the patient succeeds in interrupting the pull in any of these ways, the thigh muscles recover their activity and in this lies one of the important principles to be respected, if we hope to get these patients well without shortening. In order to pull out all the overlapping that takes place in an oblique fracture

of the thigh, the powerful *thigh muscles must be put out of commission — temporarily paralysed — by prolonged and uninterrupted moderate extension rather than temporary violent extension or prolonged severe but interrupted extension.* In the Buck's these interruptions will constantly occur, in spite of every care, unless the patient's pain is constantly relieved with morphin. In the Hodgen, as long as the limb is suspended and the cord at an angle, the pull is as constant as the law of gravitation and it will not be interrupted because the patient is comfortable and has no desire to interrupt it. If the suspension-cord is very short, the pull is straighter and the patient is more confined and the weight of the pull has to be greatly increased. Under these careless conditions the patient is made uncomfortable and evinces the same tendencies as the patient in the Buck's and allows the splint to rest comfortably against the foot of the bed. The suspension-cord of the Hodgen can be shortened until it acts upon much the same principle of the Buck's. Even with the shortest cord, however, it is not nearly so painful; the results are better and the patient has more liberty and no bed-sores. During the first week in the Hodgen splint, the patient is very much interested in avoiding the painful spasms of the thigh muscles and actually finds a pleasure in pulling up in the bed and increasing the traction, because he knows that is the surest way of avoiding them. After the first week he *keeps* himself pulled-up because he has formed the habit, because the surgeon advises him to do so and because he finds it more convenient in every way. These patients sit up in bed from the first to eat, write, bathe, and use the bed-pan, and when they lie down they can turn almost one-fourth of the way to either side. If it has been substantially applied, it is almost automatic in its action and does not require one-fourth the amount of attention from doctors and nurses necessary in other methods.

Of the three principles involved or rather of the three objects to be attained in the treatment of simple fracture of the femur, all are met perfectly by the proper use of this splint. These are:

1. The avoidance of shortening. 2. The avoidance of angulation, and 3. The avoidance of rotation which is practically always eversion of the foot. The two first are disposed of when we have pulled out all the over-lapping; as the shortest distance between two points is a straight line, of course there can be no angulation when there is no shortening; and with this splint, shortening can be eliminated in all cases. The constancy of the traction causes a rapid atrophy of the thigh muscles and the very moderate pull of eight or ten pounds, in a patient of medium adult size, will pull out all the overlapping in ten or twelve days; and by reason of the uncovered thigh and the rapid atrophy of the muscles, the formation of the callus can be followed from day to day by the surgeon's fingers. Owing to the slight irritation of the seat of the fracture the callus formation is, I am sure, more rapid than with other methods of treatment. Usually by the end of the third week, if by neglect the shortening has not been all pulled out, I have found it impossible to do so after this. In contrast to this, one case fell into my hands, that had had on a plaster splint four weeks. When this was cut off there was found to be four inches shortening. I put him in the Hodgen purely as an experiment; and by extra-weighting the limb, greatly increasing the usual angle of the supporting cord and elevating the foot of the bed and tying his shoulders to the head, at the end of four weeks more, three of the four inches had been pulled out, and he recovered with one inch shortening.

When careful daily measurement shows the limb to be as long as its fellow, the pull may be moderated: although the patient never complains of the pull. The patient frequently complains mildly during the first week or two of the heel or some other part of his limb resting too heavily on the hammock; this can be instantly relieved by loosening the canvas under that part, or tightening the part of the canvas on either side of it by means of the safety-pins along the outer bar of the iron frame. Or again, he may say his limb below the knee is sagging too low or is lifted too high; this is remedied in a moment by shifting one or two of the small tent-blocks. He *never* complains of the pull. Usually it is the mere tedium that annoys him and the slightest change of any kind will relieve him; most frequently merely raising or lowering the whole by means of the large tent-block above will satisfy him.

Rotation can be absolutely prevented by over-correcting the eversion in the manner I have described, of slightly raising the outer bar of the splint and rolling the limb over and propping it there with one or two rolls of cotton.

The chief point I wish to insist upon is, that *muscular resistance in these cases should be overcome gradually* and that it is a wrong principle to endeavor to reduce "or set" a fracture of the thigh under ether or chloroform, or expect it to stay reduced, if such reduction should be accomplished. This can very rarely be done and still more rarely can it be made to stay reduced. Such efforts are hard for the surgeon and assistants, painful to the patients and harmful from the possible trauma, and are not necessary because, with

this splint, reduction can be accomplished perfectly in two weeks with no trouble at all. The method of Bardenheuer (longitudinal and lateral traction) is complicated and requires the constant and careful attention of the surgeons and the best hospital care; and the same may be said of the extension-abduction plaster spica of Royal Whitman. Even admitting that their results are good, such treatment and after-care are out of the reach of at least four patients out of five: while this splint can be used anywhere by almost any one at little cost of time, money or work; is used without an anæsthetic unless for diagnostic purposes; gives the greatest comfort (to all concerned), the quickest recovery and by far the best results.

In fractures of the hip in old people, I and my professional friends in this vicinity have had most gratifying results. In nine cases of my own and about as many more in the practice of others that I have been consulted about, none has died and the majority have resulted in good union, little shortening if any and no stiffness of the hip or knee and no bed-sores.

This practice has been scattered over a number of years, and I have been able to get skiagraphs of only two of them. Some of the earlier cases were put up in the Hodgen, only with the object of making them comfortable and with no hope of getting union; and little or no extension was applied. Two of these resulted in no shortening; but union resulted in all but two.

Uniformly good and excellent results have been obtained in my own practice in something over 30 cases of simple and multiple fracture of the femur, but most of them were treated before we had the X-Rays, and most were in private houses, where an initial skiagraph could not be taken. Later on in this paper, I report briefly those cases in which a skiagraph was obtained. For more than 20 years we have habitually treated many other conditions than fractures of the thigh with this splint. Nearly all conditions of the limb, in fact, where rest, elevation, relaxation and the comfort of the patient might contribute to the recovery, viz., All compound fractures and dislocations, fracture of the patella, infections of the bone, soft parts or joints, resections of the hip (six cases) hip-disease and phlebitis. In such cases the limb is virtually removed from the body, so far as any movement of the body causing pain in the limb is concerned. When a painful limb is swung up in this way, it swings about and accommodates itself to every body-movement, because the limb swings as a whole between the ligaments of the hip-joint and the suspension-cord, and these are both non-resistant. All the patients, with resection of the hip recovered; and it would be difficult to convey in writing any adequate idea of the assistance given by the splint in dressing these cases, in changing the bed-linen and caring for them generally. In these and other compound cases, the hammock can be unpinned above or below or in the middle at the knee according to the situation of the wound, and, while it is being dressed, the remaining sections of the hammock support the limb more steadily and tirelessly than any two or three live assistants could possibly do it. The case of hip-joint disease (which I shall make the subject of another report) was of four months standing. The fever, pain and soreness and the starting pains had put her in the bed and three successive attempts to relieve her with plaster dressings had resulted only in her growing steadily worse. After one week in this splint, she played all over the bed and did not object to having her hip examined; and starting pains, which had kept her from sleeping for weeks, had all ceased. At the end of three weeks the fever, pains and soreness were all gone, when she left my care and went to a distant state to live.[1] I have suspected from this one experience that separation of the inflamed surfaces is the important thing, and not fixation, in the treatment of this disorder. The pull of the Hodgen separates the surfaces completely and constantly but by no means fixes the joint, and yet this child got better from the day it was put on and continued to improve in the most remarkable degree the whole time she was in my care. This child on her back, in a well-ventilated room got a great deal more exercise than she did sitting out-doors, watching the other children play while she herself was suffering greatly in the plaster.

CASE 1.—H., age, 30. In a railroad accident, eleven years ago, he sustained a simple fracture of the upper and middle third of the right thigh, and a crushing injury of the left thigh, necessitating amputation in the upper third. He suffered a good deal from sepsis and sloughing and the stump had to be dressed frequently for several weeks. This, together with the fact that his left limb had been cut off and we had nothing to compare the measurements with, and that shortening could affect only his stature any way, made us somewhat careless about possible shortening. According to the skiagraph there was overlapping, and of course there was shortening, though it is probable that most of it occurred after he got about. In one particular this splint may some times be the indirect cause of shortening. Patients lying with a limb in this splint get a great deal of exercise, so that at the end of eight weeks, when the condition of the thigh permits them to get up, the muscles of the arms, body and sound limb are always in fine condition, with not even any stiffness of the knee or hip, and they may get to putting

[1] Since writing the above another case exactly similar has been treated in this way with the same result. This patient is still in the splint although all symptoms of hip disease disappeared two months ago.

weight on the limb too soon and the callus yields. This man was on crutches by the twelvth week, and as he had only one limb and that the broken one, it had constantly to support his whole weight, whereas ordinarily the crippled limb would be protected by the other one for weeks after getting about. When this patient left the hospital in the ninth week, the atrophy of the thigh muscles was so pronounced, that I feel sure such a degree of overlapping as the picture shows would have been detected by the examining touch: but it was not, so I think the most of the shortening was caused by walking too soon and too much. The patient himself is positive this was the case.

CASE 2. (Fig. 10.) Paul D., aged 8. In April, 1907, he fell from a tree and, landing on both feet, fractured both thighs in the middle thirds. His limbs were put up in two Hodgen splints at his home, a short distance out of town. The pulley-wheels were screwed into the board ceiling about 22 inches apart, to make the limbs swing in slight abduction. The suspension-cord was about 8 feet long above the bed. Owing to the slight weight of the limbs there was, as is usual, a good deal of muscular spasm for the first week. To correct this and to give weight to the limb so that the traction would be greater, two long, loosely-filled sand-bags, weighing three pounds each, were placed on the front aspect of the thighs and held there with a few turns of a roller bandage. The foot of the bed was elevated about eight inches. When the cord was perpendicular the scales showed a weight of fourteen pounds, with the sand-bags. When the bed was moved so that the pull on the scales was nineteen pounds, or an increase of five pounds net, the rope was at an angle of about twenty-five degrees.

Having to leave the city at the end of two weeks after this, I visited this boy every other day during this time, and instructed the doctor constantly in the management of the splint and gave instructions to the mother, who was also nurse, as to managing with the bed-pan and changing the bed, etc. As the callus was being formed and the limb was straight, before I left, I put on well-padded, heavy plaster casts as an added safeguard and, in lieu of the sand-bags, to add weight to the limb. Hand-holds were placed at the head of the bed and the patient was instructed to keep himself pulled-up in the bed. This, however, is always a superfluous command as they always do so in seeking their own comfort.

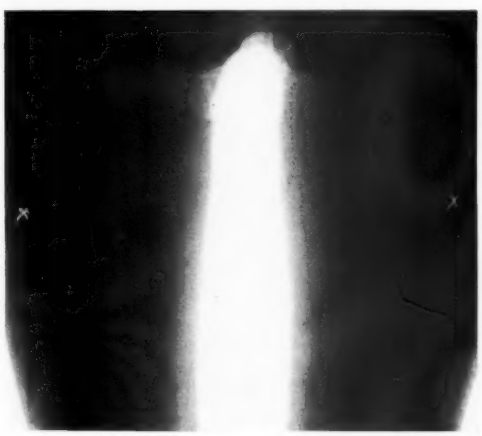

Fig. 10. Case 2. Paul D., aged seven years, fell from a tree on his feet and fractured both thighs about the middle third. Treated with two Hodgen splints. Limbs exactly the same length. Skiagraph taken six months after injury.

Fig. 11. Case 3. A. S., aged eighteen. Had fracture of left thigh in middle third nine years ago at the age of nine. Hodgen splint. No shortening or deformity. Side view. Antero-posterior view does not locate the site of injury.

I next saw this boy three months later, and his mother informed me that the doctor did not touch either splint until four weeks after my departure, when he took both of them off, and that two weeks later the patient was walking about as well as ever. The skiagraph shows the antero-posterior view. A fluoroscopic side-view showed as little deformity but unfortunately we did not think it necessary to take pictures of them. The limbs are the same length.

CASE 3. (Fig. 11.) Is that of the thigh of a young man, now 18 years old, whom I treated when he was 9. At the time, his limb was so light I extra-weighted it by slinging a coupling-pin to the underside of the hammock. He was very unruly and flounced about over the bed at all times; but the suspended limb accommodated itself to his every movement, and he was entirely powerless to get it out of fix. I hunted him up two months ago (Nov., 1907) and had an anterior skiagraph taken. It showed no defect whatever in the line of the bone, and we were so sure we had taken the wrong limb that we sent for him again to come in from the country, when we got this side-view by which one is barely able to locate the seat of the fracture. A picture of the other thigh showed a normal bone. There is absolutely no shortening.

CASE 4. (Fig. 12.) J. W., aged 35, was treated in my hospital, April 4, 1901, for simple fracture of the right femur at the junction of the middle and upper thirds; and was discharged May 21, 1901. During the seven weeks he

Fig. 12. Case 4. J. W. Simple fracture of middle third, eight years ago. Skiagraph of two weeks ago shows perfect result, and patient claims limb is longer than before.

was under my care, I am sure he did not require my attention more than ten minutes a day on the average, including the fifteen or twenty minutes it took to fix it up at first and the same time spent in giving him one only entire change of dressing so that his limb could be bathed and a clean hammock applied. During these seven weeks he required very much less attention from the nurses than a patient with a sprained ankle would. He sat up in bed the greater part of each day after the first week. He read and wrote, bathed himself and used the bed-pan without assistance, and as the weather was warm he frequently got over on another bed, rolled alongside, while his bed was turned and freshened up. He had no discomfort of any kind after the first week of his confinement. Measurements, gait and skiagraph, taken seven years after the injury, all show a perfect result. This patient is the only one I have had who says his limb is longer than it was before. Theoretically this is possible as muscular resistance is the only cause for shortening in that it prevents the over-lapping being pulled out. If the muscles should be so stretched that the over-lapping would be over-corrected and under-lapping exist, the bone would actually be longer.

Case 5. W. H., aged 65, treated by Dr. McAdory, in 1903, for fracture through the great trochanter which was demonstrated under chloroform, and loaned to me in November, 1907, for X-ray purposes. The doctor gives me the history that there was eversion and two inches shortening; that he was treated for eight weeks with my Hodgen splint, and remained in perfect health and comfort during the whole time; which the last patient corroborated. He was walking about again in twelve weeks, and had never had any stiffness of the knee or hip and no bed-sores. No shortening is to be detected at this time.

Case 6. (Fig. 13.) J. L., aged 30, was thrown from a horse and had his left hip broken in September, 1906. I saw him in consultation with Dr. C., two miles in the country. Ether was given and the diagnosis made of fracture through the great trochanter. The limb was three inches short and in complete eversion. Three hours later we swung his limb up in the Hodgen without ether and without hurting him. I visited him every day for a week, adjusting a pin here and a block there, until he was entirely comfortable and understood how to keep himself so. This he informed me was by keeping himself pulled up in the bed and so to place his body that the rope would pull his leg slightly away from the other one, which happily was also the best possible formula for the welfare of the limb. After the first ten days I did not go back to see him for three weeks, supposing Dr. C. was attending him. I found that no one had touched it since I had been there. All this time he had moved about in bed as he pleased and had been entirely comfortable. I found the splint in good condition and still doing its work perfectly and automatically, as was to be seen in the absence of eversion and shortening. There was such a great falling away of the muscles and fat in the region of the trochanter that I could satisfy myself fully that the fragments were in good position and could feel that there was a very small amount of callus, which is another evidence that the fragments are well approximated. I lowered his limb to the pillow and took every thing off; and while I pinned on a clean hammock-cloth, his wife bathed and rubbed his limb. The adhesive strips had not been made double and had stretched somewhat but, otherwise, the changing was not necessary, except for the freshening up it gave him. This was the only time the splint was changed in the eight weeks, and this took about fifteen minutes without an

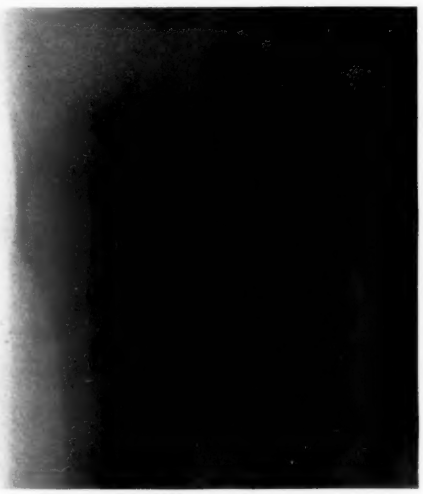

Fig. 13. Case 6. J. L., aged thirty-five, was thrown from a horse and fractured right thigh through the trochanter. When first seen, foot was in extreme eversion and the limb was three inches short. Fracture diagnosed and demonstrated under chloroform but not with X-rays. Above taken one year after injury. No shortening whatever.

Fig. 14. Case 8. Badly combined fracture of middle third, thirteen days after injury. Strong traction being made and limb being held as straight as possible while skiagraph was taken.

assistant, the union being firm enough for me to hold the foot up with one hand and slip the splint under with the other; the rest of the procedure being merely to tie the traction-strips through the distal loops to fix the limb in the splint, looping the four cords over the hook of the scales, elevating the whole by means of the large tent-block and rolling the bed to its proper position and elevating the foot again on its blocks or bricks. This case well illustrates (what I have seen too often to recall all of them now) that if this apparatus is put up substantially in the first place, and the foot of the bed sufficiently elevated to prevent the patient slipping to the foot in spite of himself; above all, if he is looked after attentively the first few days until the muscular spasm is controlled, and he is shown that the best thing for his limb is that which makes him most comfortable, this splint comes very near to being indefinitely automatic in its action. This skiagraph shows such a perfect result that one might doubt there had been a fracture, if the line of union could not be plainly seen running from the surgical neck through the great trochanter to below the lesser. This man had no stiffness of the hip or knee, and was walking well by the sixteenth week without a cane and without a limp. He has no shortening whatever: and when I saw him six months later I could not tell which limb had been injured, or that he had ever had an injury at all.

Case 7 is the final skiagraph of a man aged 30, who had a bad simple comminution of the thigh.

Case 8 (Fig. 14) is so much like Case 7 in history, course, and results, that I shall report them together. Case 7 was treated by Dr. Torrance and case 8 by Dr. Whelan and myself. These cases should not be reported as having been treated with the Hodgen splint; and I report them only as examples of "how *not* to do it." They were treated according to my recommendations in every particular, except that the hospital management would not allow a pulley-wheel to be put in the ceiling and would furnish no suspension-frame, except a very unsteady one that allowed of a suspension-cord only two feet long. I had heard repeatedly that the results at this hospital were not entirely satisfactory as to shortening, even while it was common practice to have the spring-scales showing a greater pull than I was in the habit of using. Being permitted to visit No. 8 frequently and practically take charge of his treatment, I satisfied myself perfectly that an adequate pull cannot be applied with a cord only two feet long. The greatest pull I could effect was twenty-five pounds. This was pulling almost horizontally; was very irksome; restricted the patient's movements to nothing, and invariably pulled his foot down against the foot of the bed, and still did not reduce the shortening. These conditions more nearly represent the stiff, uncompromising pull of the Buck's than the free and comfortable swing of the Hodgen. These patients have fairly good results. No. 7 is said to have no shortening; but the skiagraph undoubtedly shows that he has some, while I know that No. 8 has nearly an inch. Had these two cases been treated in their homes, with a long suspension-cord so that the patient's body-movements would actually increase the efficiency of the eight or ten pounds pull, giving that rocking teasing motion, I feel sure the results would have been much better. In fractures badly comminuted like these, a heavy plaster dressing should be applied from the knee up to the perineum to aid the natural splinting action of the tense muscles in forcing the fragments back in place. I mention these last two cases only to show the importance of the long suspension-cord: and that the more this cord is shortened the nearer the principle of extension approaches the principle of the Buck's extension, which it has been one of the main objects of this paper to explain is not the proper principle.

Another patient, R. D., aged 14, hurt in a football game in November, 1905. He was treated for a sprain of the knee until two months later, when the X-ray revealed a backward and outward fracture-dislocation of the knee. In June, 1905, I operated for the reduction of the deformity which was causing him to walk with a very bad flexion-valgum limp. The condyles or lower fragment had become firmly united to the posterior edge of the fracture surface of the shaft, and to a mass of new bone that had formed under the stripped-up periosteum of the under side of the shaft. This union was chiseled through and, then by sacrificing a thin plate of bone from either fragment, the reduction was effected and the fragments sutured with one heavy silver wire, as shown by skiagraph No. 20, taken eight weeks afterward. The operation was done without opening the joint, the wound was closed with silk-worm gut and a small drain of the same, and the limb put up in the Hodgen without extension. There was some infection and suppuration; and the wound had to be dressed daily for a week or two. By having the knee portion of the hammock a separate piece, this was unpinned and turned back making the dressing a very easy matter for one person with no assistance, except that given by the thigh and leg portions which supported the limb while the dressing was done. This patient suffered little or none and was out in eight weeks with a perfectly

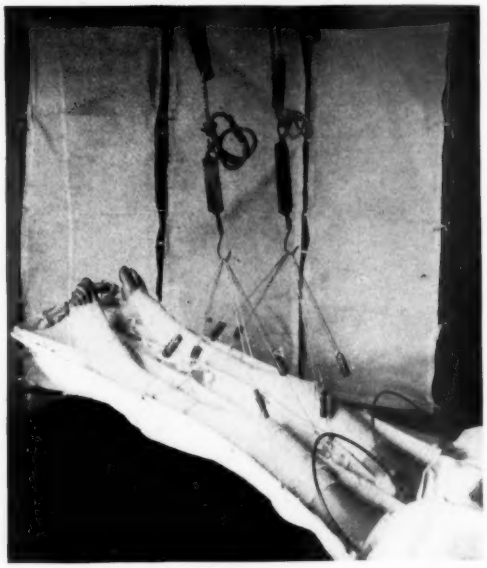

Fig. 15. W. G., aged 56. Compound comminuted complicated shattering of the condyles of both femurs; knee joints torn widely and filled with mud; both patellar tendons cut through. Cleansed, repaired, and swung up in Hodgen splint.

straight limb and one inch shortening, due to loss of bone and growth in the epiphysis.

One case of compound fracture of the femur, with severe infection and sloughing, in which from first to last more than twenty incisions were made in the thigh and gluteal region, the whole septic period lasting four weeks, was finally saved with one inch shortening due to loss of a fragment of bone. In this case, large hot wet dressings were applied every four or six hours, frequently by the night nurse alone. In doing this the limb was elevated by means of the large tent-block and pulley, and a large douche-pan was slipped under the buttocks. With the thigh-half of the hammock unpinned and turned down, the upper portion of the splint would tilt up out of the way and the leg portion of the hammock supported the whole limb while the dressing was applied.

Photograph 15 is that of a man who sustained compound comminuted fractures of condyles of both femurs, both so extensively shattered they could not be wired, patellar tendons cut across and both knee-joints torn widely open and ground full of mud. By having the hammock in three pieces and turning back the knee portion, the wet dressings could be so easily and frequently applied that this patient never had any septic absorption at all and recovered, in part, with one stiff limb and with some motion in the other. The latter limb was inadvisedly amputated nearly a year later, however, on account of continued suppuration of the shattered condyles.

Lastly, I have two old lady patients, eighty years of age, whom I treated six and ten years ago, respectively for fractures of the hip. Neither of them had any stiffness of the knee or hip; and both have been walking about ever since, with no apparent shortening. Both are too feeble now to leave home to have their skiagraphs taken.

A few other cases, I might have presented from the practice of my surgical friends and said to be good results, were open to the objection I have dwelt on above, i.e., a very important principle had been neglected or ignored; and I did not consider such cases as fair examples of the method.

After using this splint for more than twenty years in its gradual modifications and giving it a good deal of thought in that time, I am now inclined to believe and make this seemingly radical statement, that there is no simple fracture of the thigh that cannot be successfully treated with it, and that it is never necessary to do an open operation in a recent case.

If the details of the method I have described are carried out, the results should be perfect in nearly if not all cases, and the amount of attention required from surgeons and attendants will not be one-fourth that of other methods. If it is put up in a slovenly manner, it will not be substantial, the work will be annoying and the results may be anything.

The greatest difficulty is, that accurate attention to the details is not given. While these are not at all difficult nor complicated, they do not *seem* to be of much importance and are often slighted by the beginner when they are really vital to the success of the whole. If a tent-block slips or splits, the cords of traction-strips stretch, the pulley pulls out of the ceiling or the bricks supporting the foot of the bed tumble over, the operator with any mechanical bent at all will be able to locate the defect and remedy it without getting discouraged to the extent of abandoning the experiment altogether.

One of the leading surgeons of the country began an article on fractures a year or so ago, by saying (in effect) that after a very large experience in accident surgery he still approached the treatment of fracture of the thigh with much misgiving. In contrast to that, I have to say there is no fracture in the treatment of which I am so confident of

getting a good result. By a good result I mean: the patient made comfortable, a minimum amount of work for the surgeon and attendants, no bedsores, the patient retaining a good muscular condition by reason of the great amount of liberty of movement and exercise he gets while under treatment,never any stiffness of the knee-, hip- nor ankle-joints, the quickest union and shortest convalescence, a straight limb, with no shortening and with never a non-union.

I am perfectly willing to be thought radical, even absurd by some, if I can induce a competent and influential surgeon here and there to carefully investigate the matter. The subject is certainly of importance, when it is admitted on all hands that, so far, the treatment of this fracture has not been satisfactory even in the best hands in the best hospitals; and interest should certainly not be taken from it, when I assert that it is being treated most successfully all over this part of our state; in log cabins by country friends of mine, and in the large contract-work of the mines in this district; as well as by all except one or two of the doctors in our city. I think any one can use it who has mechanical eye enough to see how the weight of the limb is used to make the extension. The rest lies in understanding the importance of putting together properly and substantially what, at first sight, seems a sort of childish plaything, and retaining one's patience until one masters the slight art to adjust the safety-pins, cords and tent-blocks, the angle of the suspension-rope and the position of the bed, until the patient is comfortable and the limb is being pulled sufficiently and in the proper line.

SIMPLE HYSTERECTOMY IN SIMPLE CASES[1]

By ALEXANDER HUGH FERGUSON, Chicago

In presenting a description of a simple operation for hysterectomy, allow me to picture a typical case in which it is indicated.

A woman, aged about 40, suffering from fibroids of the uterus of such dimensions and number as to destroy all functional activity of the body of the uterus. The ovaries and Fallopian tubes are normal. Simple or multiple myomectomy is out of the question. The cervix is either normal as in a maiden, or sufficiently in a healthy condition as not to be removable from a functional, anatomical or pathological viewpoint. Even when the cervix is not conservable, the principles of the "simple hysterectomy" should in many instances be practiced. Indeed from a case with a comparatively limited pathology to the extreme conditions where the possibilities of this operation being performed, the indications widely vary according to the capabilities and knowledge of the man behind the knife as well as to the conditions encountered.

As a rule the vagina and cervix uteri should be rendered surgically clean. When the hymen is intact and there has been no history or signs of infection this is not necessary.

1. Leave the cervix.
2. Leave the ovaries.
3. Leave the tubes.
4. Leave as few absorbable sutures within the abdomen as possible. Never use silk or linen or cotton material for sutures.

OPERATION

Prepare, anæsthetize, and laparotomize the patient. Drag the pathologic uterus out of the abdomen, protect the omentum and bowels with moist gauze pledgets of suitable size and thickness. Crush the broad ligament close to the uterus on both sides. The instrument used for this purpose must be very powerful and, if possible, the uterine artery is included in its bite. Remove the clamps and insert No. 2 chromic catgut and forcibly tie it in the crushed track on each side. As an additional precaution against slipping of the ligature, one of its ends may be passed under the round ligament externally and tied to its fellow on the inside. Apply a strong crushing forceps proximal to the ligature on the broad ligament on each side and cut between them. Fig. 1.

The forceps prevent bleeding from the uterus and growths, while the ligatures produce hæmostases from the vessels in the broad ligament. A separate ligature (No. 0 chromic catgut) may be applied to the cut vessels distally as an additional permanent security against hemorrhage.

Amputate the uterus above the cervix (Fig. 1 and 2), leaving fore and aft liberal flaps. Rim out the internal os uteri, and in women who have borne children or when the mucose of the cervix has been curetted, pack formidin or iodoform gauzes into the vagina from above downwards. On threaded, strong, round needles, pass the ends

[1] Read before the Chicago Gynecological Society, February 21, 1908.

of the ligatures from the broad ligament stumps through the base of the flaps of the cervix on each side of the cervix (Fig. 2), tie them and sew the flaps of the cervix over the broad ligament stumps with two or three interrupted sutures (Fig. 3), or by a continuous suture, which completes the operation with the exception of closing the abdominal wall.

It is often necessary for perfect coaptation to employ both deep and superficial sutures. For this purpose one suture may be inserted, one portion passing deeply and the other superficially as in Fig. 2. This double function (superficial and deep coaptation of the structures) of one piece of catgut has a wide range of application in surgery. It is my favorite method of closing the muscle and fascia of the abdominal wall. Either continuous or interrupted suturing may be employed with equal facility.

The conditions that vary in different instances demand a variation in the technique of the operation, which of course any surgeon does according to his own sweet will, but the objects aimed at must stand inviolate. These aims are:

1. A minimum number of sutures.
2. A minimum amount of raw surface.
3. A prevention of prolapse of the cervix and vaginal vault.

I have performed this operation fifty-two times without a death or complication, and without any dissatisfaction to the patient.

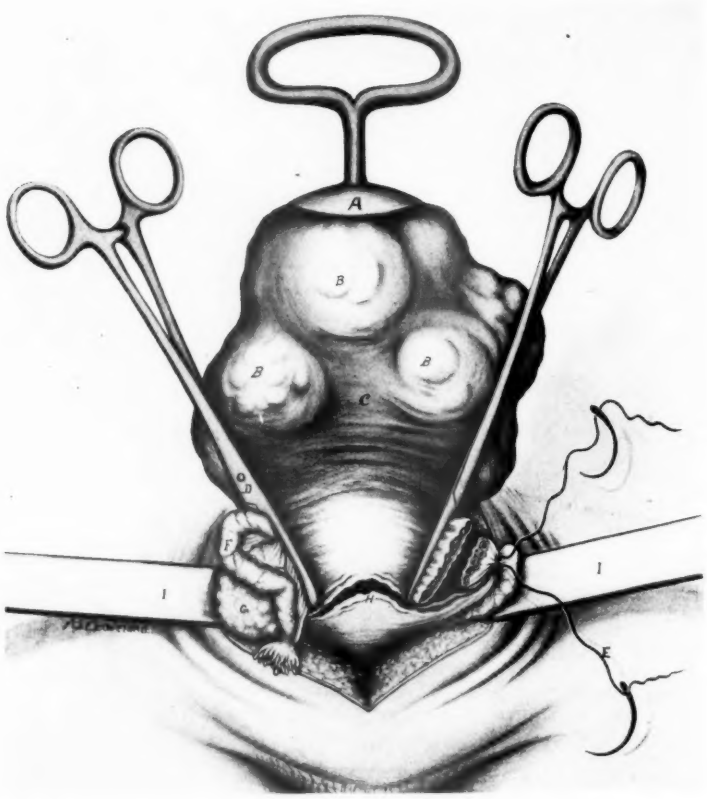

Fig. 1. *A*. Corkscrew. *B, B, B*. Fibroids. *C*. Uterus. *D*. Clamp. *E*. Ligature tying off broad ligament and vessels. *F*. Fallopian tubes. *G*. Ovary. *H*. Anterior incision for supravaginal amputation of uterus.

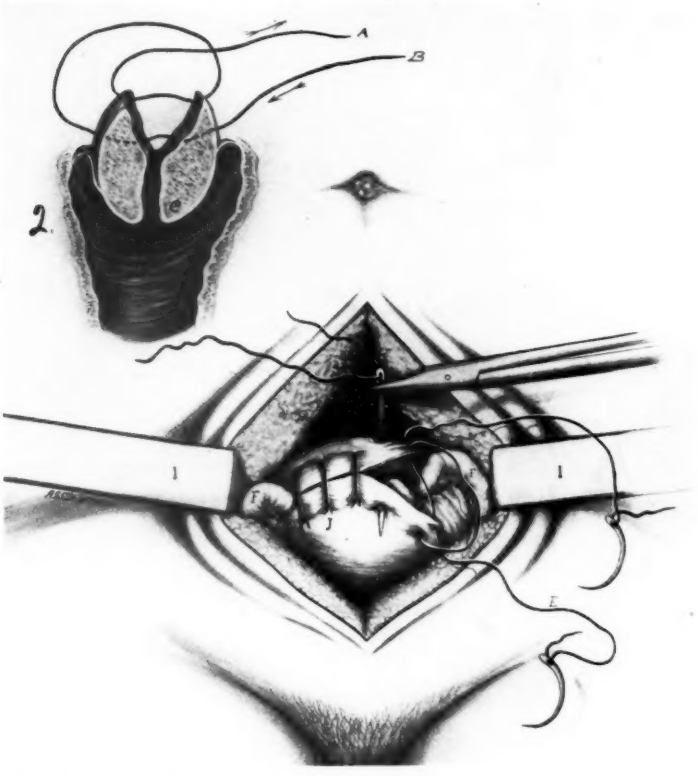

Fig. 2. *F, F.* Fallopian tubes. *J.* Stitches closing the stump. *I.* Retractors. *E.* Ligature tying off broad ligaments and vessels.

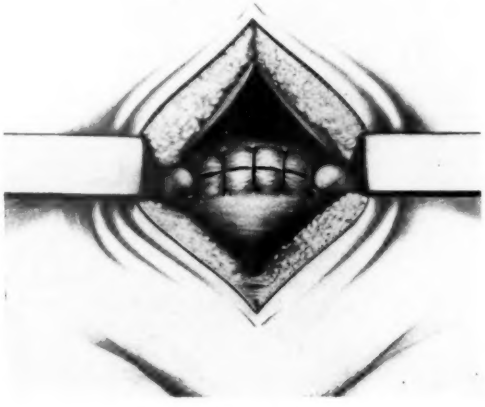

Fig. 3.

TRANSACTIONS OF SOCIETIES

MEETING OF THE CHICAGO GYNECOLOGICAL SOCIETY, FEBRUARY 21, 1908

J. B. De LEE, M. D., PRESIDENT
FRANK W. LYNCH, M. D., EDITOR

EXHIBITION OF SPECIMENS

DR. HENRY F. LEWIS: This kidney is from a man about thirty years old, who entered my service in Cook County Hospital after having suffered a fall upon the right side. He was in a condition of shock and was acutely anæmic. There was found upon examination an area of flatness extending from the groin up to the liver and over to the umbilicus. He was in an extremely bad condition. The pulse was high, temperature only slightly elevated. I opened the outer border of the right rectus at about the level of the umbilicus, running an inch below, and about three inches above, and came down on this large mass, which was post-peritoneal, and consisted of this cystic kidney. Inside its capsule and outside in the fatty tissue around the kidney, was a large amount of clotted blood. It was evidently a rupture of some portion of the kidney. There was an immense amount of bleeding after the kidney was removed, in spite of ligaturing the vessels, so it was necessary to tampon the cavity. The patient survived only about twelve hours.

The interest is from a pathological standpoint, and consists in its being a multilocular cystic kidney. The other kidney was in a normal condition, as far as I could tell by feeling.

The second specimen came from a case where I performed supravaginal amputation of the uterus for a large cystic myoma of the posterior wall.

A large mass lay within, and connected with the uterus, and as large as a full-term fœtal head. At the time of operation, three-fourths of a pint of bloody serum was obtained by the trocar through this thick-walled cyst. There are several small fibroids in the uterine wall, one to the left of the fundus about as large as a walnut. I took this tumor to be an ovarian cyst, probably a dermoid. At the examination, the cervix was quite long and this, the fundus, appeared to be distinct from the tumor. On opening the abdomen, the first thing that came into view was a pair of fairly large hydrosalpinges. Each has diminished at least 75 per cent in size from immersion in the preservative. They were about 4 inches long and 1½ inches in diameter. They came up into the wound and I removed them first, then tapped the uterus with a trocar, and obtained three-fourths of a pint of bloody serum. The woman was 45 years old, was still menstruating rather normally, but had noticed for the last two years a gradual growth in the abdomen. There was no sudden growth at any time. I had some slides made, but they are not in a condition to use. They show a specimen taken from the wall of the cyst, apparently granulation tissue on the inside and ordinary fibroid structure, with nuclei staining very slightly. Probably the hæmorrhage was the starting-point of the cyst. There is no sign of sarcoma.

DR. J. B. De LEE: Three weeks ago Sunday, a patient entered the Lying-in Hospital, at the eighth month of her third pregnancy. She had felt very well up to the morning of her admission. She says she always menstruated very profusely, and her general appearance before this time had been one of more or less anæmia. Sunday morning she had a severe pain on the right side, low down near the appendix, which, however, did not prevent her from pursuing her housewifely duties. She was compelled about noon, on account of excessive pain, to go to the hospital, and on arriving there was in a state of shock and anæmia. Her respiration was sighing, pulse very weak, but not fast. The pallor was extreme, and external hæmorrhage had been going on for an hour or hour and a half. Fœtal movements had not been felt the morning she arrived at the hospital. Owing to untoward conditions, help did not reach her until 4 o'clock. When she arrived at the hospital about one P. M., she was examined by a physician who happened to be present, and when I arrived at 4 o'clock, the patient had rallied somewhat from her first hæmorrhage and was resting fairly well, but complained of intense pain in the abdomen and more or less exhaustion. Examination showed the uterus very much distended, as large as a nine months; pregnancy, in a state of constant contraction, the walls tense and

very tender, especially on the right side low down, but there was no irregularity. Vaginally, blood was escaping in a continuous stream. On internal examination, the cervix was found partly effaced, admitting two fingers, rigid, and very high up in the hollow of the sacrum. A diagnosis of premature detachment of the placenta was made, and the patient put on the table for vaginal Cæsarian section. The pulse had gone up in rapidity and had not decreased much in strength, although it had very small volume. The yawning had recommenced and the patient was in a critical condition. The vaginal Cæsarian section was attended with unusual difficulties. It was impossible to bring down the anterior wall of the uterus; it was impossible to bring the cervix down to the vulva, or anywhere near it, even after the transverse incision had been made in the usual manner. When I attempted to bring down the anterior wall of the uterus by the vulsellum, a piece of the uterus pulled out. The blood coming from the uterus itself, plus the blood flowing from the wound of operation, interfered very much with the incisions; it was almost impossible to see anything, and the wound itself bled very profusely, confirming our suspicion that we we had to do with a woman of bleeding tendencies. It was necessary to complete the operation under the guidance of the fingers, by touch, which was accomplished and the baby extracted up to the head. It being dead, a perforation was made in the skull, the cranium emptied, the placenta delivered, and the whole genital tract tightly tamponed. The woman became pulseless on the table. I forgot to mention that the entire operation was done without an anæsthetic, owing to the woman's extremely grave condition. While she made some noise, she remained perfectly quiet, appreciating her condition. After packing the uterus, we gave the woman salt solution by the rectum and hypodermically. The amount of salt solution is of considerable interest; she received two quarts and a pint per rectum and two quarts and a pint by the skin, and rallied so that she was in pretty fair condition when I left. At midnight she began to get worse, the pulse disappeared, and she got two quarts more, making a gallon by the skin, and another quart per rectum. When I arrived at two o'clock the woman had not rallied sufficiently, so I gave her another quart and a pint per rectum. She received a total of a gallon and a pint through the skin, and a gallon and a pint per rectum. She retained all this salt solution, and it seemed to keep the pulse going. She lost consciousness after 9 o'clock, lost all knowledge of her surroundings and paid no attention to her treatment until 6 o'clock, when she awoke, claiming she had slept all night. At 3 o'clock the oozing had recommenced, so I injected into the gauze a two per cent solution of gelatin and packed the vagina very tightly with cotton soaked in gelatin. There was no hæmorrhage after this, the patient became fully conscious, and while she was completely blanched, has made an uninterrupted recovery.

The points of interest are: 1. The difficulty of drawing the cervix down so it could be reached in the usual operation of vaginal Cæsarian section; 2. The friability of the cervix; 3. The hæmorrhage from the tissues themselves; 4. Hæmorrhage from the interior of the uterus; 5. Old hæmorrhage blocking the operation, which was done under the guidance of two fingers; 6. The immense amount of salt solution administered. Another point; in the first twelve hours after the operation only one ounce of urine was passed, and that nearly solid albumin, and in the next 12 hours ten ounces were passed, making 11 ounces in the first 24 hours. Possibly it was due to the action of the salt.

The other case was acute toxæmia in pregnancy, with threatened eclampsia, with all the urinary indications that go with such a case: albumin, blood, casts, red and white blood cells, a retinitis gravidarum, or rather retinitis albuminurica, severe headache, all the symptoms of an impending eclampsia. In this case, a pregnancy of eight months was also found and a vaginal Cæsarian section was also done. The cervix was about the same size as in the last case, admitting two fingers and partly effaced. This woman was a primipara. In this case the cervix was easily brought down to the vulva, the vaginal hæmorrhage was not to great and the operation was completed in the usual way, without any trouble whatever. As soon as the placenta was delivered the woman began to bleed, not alone from the uterus but from the incisions. It is curious, and I do not quite understand why we were not troubled more with hæmorrhage during the operation. The amount of blood that came from the incisions was not much more than usual; but when it came to sewing up the peritoneum; the oozing was troublesome and was stopped only by tamponing. In removing the tampon the next day, the hæmorrhage recommenced from the abraided surface of the vagina and in the incisions of the uterus, so it was necessary to tampon again. This woman gives a more accurate history of a bleeder. She says a tooth extracted gave a great deal of hæmorrhage; when she cuts her finger, it takes a long time for the hæmorrhage to stop, and she also has a profuse menstruation of eight to ten days. This woman also made a satisfactory recovery, but the child died from an acute intestinal affection.

DR. DE LEE: Dr. Bacon, have you met with difficulties such as I have described?

DR. BACON: No, I have not. Have you any idea why you had difficulty in pulling down the uterus?

DR. DE LEE: I have not the slightest idea why. (Illustrating) There is the sacrum, there is the pubis: the cervix was away up there, and it would not come down. The woman had no history of difficult labors, and no tumor, no adhesions of the uterus, but it was impossible to bring it down. I did not use a long speculum, but a short one which did not go in farther than that, so there was no long speculum to push the cervix up into the hollow of the sacrum. She was a multipara, and one would expect the cervix to come down easily. The next case was a primipara, and the cervix did come down easily. I would also like to get an expression as to the hæmophilic tendencies of the case.

DR. G. KOLISCHER: In the first case that Dr. De Lee reported, it is rather questionable whether vaginal Cæsarian section was indicated. The essayist himself reports that it was impossible to pull down the cervix, so that he had to operate entirely led by his sense of touch. Now to make a Cæsarian section entirely in the dark is not an ideal procedure. The indication for it becomes even less understandable, if we consider that at the time of his interference the child was already dead. It would seem that bloody or unbloody dilation of the cervix, and embryotomy would have suggested themselves.

As to the second case, I would like Dr. De Lee to explain what he understands, or rather what we should understand, under the term that he uses, that a patient is threatened with eclampsia. Eclampsia, so far, is not a well-defined pathological entity, and a clinical picture only. There are no characteristic prodromal symptoms that would indicate that eclampsia has to be expected. With the same right I could say that every woman, provided she is able to conceive, is threatened with eclampsia, but how are we going to diagnose eclampsia in advance. To base an indication for operative interference on such vague premises does not seem rational to me.

As to administering excessive amounts of saline solution, I would be rather afraid to overburden the heart, and would prefer to use smaller quantities of saline solution with a few added drops of adrenalin solution, — a procedure which has repeatedly been proven to be an excellent means of tiding a weak heart over a critical period.

DR. HENRY F. LEWIS: I question the necessity for these forcible deliveries in the cases of eclampsia, or nephritis, or toxæmia, especially a delivery so forcible as that by vaginal Cæsarian section. We know that in a great many cases of eclampsia, or of toxæmic symptoms, the patient recovers after the puerperium or gets better after the child dies; and are we justified, except in very rare cases, in going so far in forcible delivery as to do vaginal Cæsarian section, especially where the operation is attended with great difficulty? In the first case there was difficulty in doing the section; in the second case there was a history of hæmophilia; and in either case it was evident that operation would be difficult and dangerous. Fortunately, I have never had to do vaginal Cæsarian section, but I remember one very interesting case at the County Hospital where I was prepared to do it, got all ready, but on examining, found that cervix dilated without trouble. That is the only case where I have been tempted to do this operation. I wish Dr. De Lee would dilate a little on the point as to what cases are so bad in regard to toxæmia as to demand such a serious operation as vaginal Cæsarian section. And why vaginal, in preference to abdominal, section?

DR. RUDOLPH W. HOLMES: Some experiments carried out in the laboratory of the College of Physicians and Surgeons of New York by Dawbarn some ten years ago, and substantiated later by others, showed that in cases of hæmorrhage it was necessary to give nearly twice the amount of saline solution as the estimated amount of blood loss, in order to get the full effect. If, for example a woman loses two quarts of blood, to carry out the recommendations suggested by the experiments, four quarts of the solution should be given. It is self-evident that the term "bleeding to death" does not literally mean that all the blood has escaped from the vessels: approximately only half of the total blood may pass from the circulatory system in deaths from hæmorrhages; the anæmia superinduces vasoconstrictor paralysis; the resultant enormous vessel dilatation means partly empty vessels. The salt solution is given with the purpose of filling the vessels with diluted blood until the hæmatopoietic function is fully re-established. I have had eight, if not more, severe cases of ablatio placentæ, and find that the amount of blood which escapes before or during delivery bears but a small proportion of that which has entered the uterus; where the presenting part is above the girdle of contact the large losses of dead blood escaping from the genital tract may be very disconcerting. I am convinced that vaginal Cæsarean section should be carried only out when the field of operation may not be obscured by bleeding from the uterine cavity — to attempt the operation, when the incision must be made blindly,

is nearly folly. I am further convinced that manual dilatation is the most appropriate method of dilating the os in ablatio placentæ, for the tactile sense alone need be considered.

About adrenalin: It has been shown by experiments that to give a few drops of a 1 — 1000 adrenalin solution merely raises arterial tension momentarily. The only logical way to give adrenalin in hæmorrhage cases is to give *slowly* a very dilute solution; by this means the pressure is continuously raised until reaction sets in.

Dr. Preble told me of a man sick with pneumonia; he was in extremis and death was momentarily expected. He was given an ordinary full dose of adrenalin — with a bound, the pressure rose, and almost at once it again fell to almost, if not to, zero.

Dr. Charles S. Bacon: I think these are very interesting cases and bring up some valuable points. The recital of the first case impresses me with the importance of the salt solution injections. Probably nothing else would have saved this patient. A woman of average weight has four to five quarts of blood, about one-thirteenth the weight of the body. We know that it is a very serious matter for any animal to loose half the blood in the body. I should judge from the report that nearly half the blood of the woman was lost in this case, and the filling of the vessels, the keeping up of the circulation pressure by the injection of salt solution was absolutely necessary. I doubt very much whether anything else could have saved her, and this management fortunately justified itself in the results.

I have taught that a vaginal Cæsarian section, *when it could be performed*, was the ideal management of cases of detachment of the placenta when the cervix was so closed that dilatation was impossible. I presume, in this case the operator concluded that it was impossible to dilate sufficiently to reach the child. If the cervix can be dilated and craniotomy done, that will save a certain amount of traumatism and perhaps of blood; but when dilatation and immediate delivery is impossible, one must make a Cæsarian section, or rely on the tampon. It can be done ordinarily in a short time and with a saving to the patient. So I should be inclined, not knowing or anticipating beforehand that there would be difficulties in the operation, to take the same procedure that was done here, and I am not sure that this case would change my opinion in similar cases in the future.

As to the second case, nothing was said in the report about prophylactic measures, and I believe these should be taken. I am inclined to assume that they were taken here. A patient with such symptoms as those described is in an extremely toxic state, and I see no objection to the statement that the patient is threatened with eclamptic seizures. She is in an eclamptic condition, but whether the seizure will come or not we cannot say, but there is danger. If she is treated with elimination, perhaps, or other methods, for a few days without results, then certainly some way of emptying the uterus is indicated. Whether it should be a slow delivery, the introduction of a bag, rupture of the membranes, or whether it should be section, must be determined. Which is the most conservative, is the only thing. It is rather probable that the rapid emptying of the uterus would give a greater chance to the child, and the rather long induction of labor would perhaps be as harmful to the mother as the rapid emptying of the uterus by vaginal section. I believe vaginal section is very much less shock than abdominal section; so, in spite of the complications in these two cases, I should be inclined to try the same procedures.

Dr. G. Kolischer: As to the remarks of Dr. Bacon and Dr. Holmes, I would like to say that they tried to refute statements that were never made. I never said that pure adrenalin solution should be administered to the patients, but I suggested, as it is very well known in the surgical world, the addition of a few drops of adrenalin solution added to the saline solution; Dr. Holmes evidently overlooked the publications referring to that. That the mere introduction of the fluid into the body in large quantities is not the only saving factor, was proven twenty years ago by the experiments conducted by Maydel.

In regard to Dr. Bacon's remarks, I never claimed that the rapid evacuation of the uterus in eclamptic cases was not the proper procedure; on the contrary, some years ago we had a discussion on eclampsia and its therapy before the same society, and I, basing my statements on large statistics, advocated this energetic interference in eclampsia. In this point of view I was strongly opposed, and one of the leaders of this opposition was Dr. De Lee. To-night, I simply said that I could not approve of doing a vaginal Cæsarian section before the diagnosis of eclampsia is made. The diagnosis of eclampsia is based on certain clinical features, that is, if a pregnant woman, or a woman immediately after the delivery, is seized by convulsions that are accompanied by loss of consciousness, and rise of temperature. So far, nobody has succeeded to prove a definite connection between nephritis and its clinical emanations and between eclampsia. On the contrary, the most experienced obstetricians admit that nephritic

patients very rarely, or almost never, are seized by eclampsia, while they know of lots of cases in which eclampsia occurred without any signs of nephritis.

I would like to call attention to the modern theories for explaining eclampsia, which all consider the circulation of toxines, the appearance of which is not dependent on the co-existence of nephritis. I therefore cannot say that I am convinced that even Dr. De Lee, with all his experience and all his decisive ways, is able to divine an eclampsia some time ahead. While I admit that a vaginal Cæsarian section is a valuable operation in its proper place, I cannot approve of the indications that Dr. De Lee propounded for it to-night.

Dr. J. B. De Lee, in closing the discussion, said: Many points have come up that are certainly discussable. First, is it possible to foresee an eclampsia? I think it is not alone possible to foresee eclampsia, but that it is actually demanded of the obstetrician to foresee eclampsia. The eclamptic convulsion is simply one symptom of the so-called eclamptic state, and when we see the eclamptic state beginning and advancing in spite of treatment, it is reasonable to suppose a convulsion is the next sympton to be expected. If you will take the histories of all eclampsias and study them carefully, you will see that œdema, headache, ocular disturbance, muscular twitching, disturbance of the nervous system, disturbance of the sense of smell, taste, hearing, etc., almost invariably precede the convulsions. Also, examination of the urine will show a decrease in amount, with the presence of albumin, casts, and blood — all the signs of acute nephritis. I am perfectly aware it has been said that eclampsia may occur without these premonitory symptoms, but in 58 cases that have come under my observation, not a single one has occurred without plenty of warning in the way I have described. I am aware eclampsia may occur without renal findings; Ingersoll is said to have quoted 108 cases. Jaggard, who had an immense experience, always doubted the accuracy of Ingersoll's cases. In my own experience there was only one without renal findings. These things, in my experience, are true: When a woman begins to develop the symptoms of an eclamptic state and has evidences in the urine, I think it is perfectly reasonable to say she is threatened with eclampsia. That one can say a woman has eclampsia only when she has a fever, is too narrow a view of the subject, because a woman can have eclampsia, and die of it, without having fever. It must be admitted, however, that most cases do have fever. Eclampsia is not a distinct entity, it comprises several conditions. I believe it would be a very unfortunate dictum to go out from this society that eclampsia is only eclampsia after convulsions have occurred, and that no man can say a woman is going to have eclampsia unless she has it, because it will take the confidence of the physician away from his examinations of the patient and the urine, and will lead to carelessness and simply waiting for something to turn up. One of the most important functions of the obstetrician is to prevent eclampsia and allied toxæmias, but unfortunately he is negligent in his duty.

Regarding rapid delivery in threatened eclampsia, Dr. Kolischer admits that after a woman has had convulsions the proper thing is to deliver her. Jaggard in 1891 and 1892 said he had gotten better results from expectant treatment, or medical treatment, but later on changed his views and leaned more to active treatment. Statistics will show that the more rapidly the uterus is emptied after convulsions begin, the better the prognosis. Therefore, in eclampsia the delivery should be as rapid as is consistent with the safety of the mother. I believe it improves the chances of the mother and of the baby. If a man believes that eclampsia is on the verge of occurring, and rapid emptying of the uterus will tend to cure eclampsia, should he not rapidly empty the uterus, when eclampsia is threatening?

I agree with Dr. Bacon that the prolonged induction of labor — and some are very long, lasting two or three days — is more dangerous than a rapid delivery by cutting or other means, rapid dilatation, for example. Therefore, when eclampsia is feared, or if the woman presents a toxæmic state that indicates the imminence of convulsions, rapid delivery, to my mind, is the best way. I asked Dr. Bacon whether abdominal or vaginal section should be selected, and he says vaginal section in the marked cases. I selected vaginal Cæsarian section because I expected not to have trouble, and in the first case I met all sorts of trouble, but in the second case it was very easy. The post-partum hæmorrhage was easily managed, and the woman did not lose an excessive amount of blood. I would even say that in a special case where vaginal Cæsarian section was evidently going to be difficult in eclampsia, an abdominal section would be justifiable. As to cervical incisions, this woman's cervix was not dilated and admitted only two fingers. Incisions in this case would have been dangerous, especially as the cervix could not have been brought down for the scissors. The cervix was thick and only partly effaced. I perform cervical incisions where the cervix is effaced, but four cervical incisions are not much safer than a clean vaginal section. Vaginal section is quicker than

dilatation by the hand; I would also consider vaginal section safer than four incisions with subsequent craniotomy.

I have never used adrenalin in cases of hæmorrhage, I am waiting to get more statistical experience. I have used immense quantities of salt solution. Dr. Reed will remember a case of rupture of the lower uterine segment, in which the woman lost an immense amount of blood, was almost exsanguinated, where we packed the uterus and sewed the cervix together over the packing. We gave the woman a gallon of salt solution through the skin and she absorbed it rapidly.

In answer to Dr. Bacon's question as to prophylaxis of the threatened eclampsia, it was a consultation case, the woman had been under the care of her family physician for three weeks, getting worse all the time. Her principal complaint was severe headache, blindness, twitching of the muscles, and indications of a nervous explosion.

Just how salt solution acts I do not know, except that I believe it fills the vessels and gives the heart something to pump on. Given under the skin there will not be enough absorbed to load up the circulation, but, given in the veins, there is a possibility of overloading the heart.

It would have been much easier to have done an abdominal Cæsarian section, because of the difficulty of performing vaginal Cæsarian section at the ends of the fingers. I agree with Dr. Kolischer that it is difficult and dangerous, still the educated finger can do a lot, and it was possible, and not hard, to get the wall of the uterus between the fingers and feel the bladder and peritoneum, the peritoneum with the index and the bladder with the middle finger, and with the scissors cut between them. it was done safely, there was no injury to the bladder or peritoneum. I would not, however, advise that method of doing vaginal Cæsarian section.

Dr. Ferguson then read a paper entitled, A Simple Hysterectomy for Simple Cases.

Dr. A. Goldspohn: I wish simply to say that it is a nice technique, but it is a little too simple. I have always had an aversion, on theoretical grounds, to leave a useless tube, an organ lined with mucous membrane that will not obliterate but may continue to secrete. When I remove a uterus, I always very carefully remove such tubes completely. Not many cases will permit drawing the parts so far out of the body as these illustrations indicate; nor is it necessary in order to remove the tubes with the uterus with very little more care or labor. Then again, these woman are usually elderly women where we remove such uteri and tubes, and I do not think it is best to leave both ovaries. While there is no particular objection to leaving them, if they are perfectly healthy, still their secretory functions in elderly patients is a matter of lesser importantce, being sooner destined to cease anyhow; while on the other hand, neoplasms, sometimes of the malignant nature, may originate from such ovaries, in still later years. Therefore, it appears to me such a patient will be rather better off with one ovary, which answers every purpose for her than with two.

Dr. Channing Barrett: Simple removal for disease, is a very correct principle of medicine. Yet we cannot say this hysterectomy is more simple or less simple, because the tubes and the ovaries are allowed to remain. That depends upon the condition of the tubes and ovaries. If one chooses to leave them, or take them out, the operation can be made just as simple. I hardly think Dr. Ferguson bases the question of simplicity on leaving the tubes and ovaries. Clamping a broad ligament with one or two clamps and tying in one ligature, is not necessary. There are four places which we should ligate before we remove the uterus, the two ovarian arteries. and the two uterine arteries. We elevate the uterus, and with a small clamp, catch the ovarian artery, then go down the side of the uterus till we come to the uterine artery. We do not have to seize the whole of the broad ligament with one clamp, or tie the whole of it in one ligature. Another point: The use of an instrument that bores into the uterus, is rather risky, because every now and then we run across a cyst that has pus in it. I removed one not long ago with an instrument like that and let out pus in the abdominal cavity. We do not know when we are going into an infected uterine cavity, and there is no need of having an instrument that bores into the uterus.

Dr. G. Kolischer: Although it is rather hard to discuss an operation without having witnessed it, I think I understood Dr. Ferguson. that the main points that he claims for his procedure is, to put clamps on the broad ligaments before severing them, and that he afterwards draws the stumps of the broad ligaments together over the cervical stump. As to the use of the clamps, a method that was suggested years and years ago, I want to endorse it. If nothing else, the application of these large clamps prevents any subsequent venous hæmorrhage. As to the figure eight suture that Dr. Ferguson uses, I don't think that he claims any particular merit for that. Whether or not the drawing together of the stumps of the broad ligaments is able to prevent subsequent prolapse, is open for all kinds of speculation. I, for one, don't believe it. The statement that was made by one of the gentlemen, that a woman that lost the body

of the uterus is better off with one tube and one ovary than with both of these organs, does not sound rational to me.

DR. A. H. FERGUSON, in closing the discussion, said: I have very little to add. In regard to the corkscrew, though not under discussion, let me say that in my cases it was all right, and for Dr. Barrett's case it was all wrong to employ it. Notice that the corkscrew has a flange which, if screwed down, prevents blood getting into the field of vision, and, I presume, would prevent pus from escaping if there were sufficient hold on the tissue. I do not use the corkscrew in pus cases, nor do I then perform the operation now described.

The question as to whether the Fallopian tubes should be removed or not I have not touched on, and will not say anything about it, nor of the ovaries. I have taken it for granted that in simple cases of fibroids, if these were normal, we should leave them alone, the same as we leave the cervix alone, when it is normal.

The least amount of suturing is done by fastening the broad ligaments inside the stump of the uterus. The manner of securing the vessels I do not care anything about, but bringing the broad ligaments together in the center and fastening them in the stump of the uterus will raise the stump up and prevent prolapse.

A JOINT MEETING OF THE CHICAGO MEDICAL AND THE CHICAGO GYNECOLOGICAL SOCIETIES

FRANK W. LYNCH, M. D., EDITOR

A joint meeting was held, March 25, 1908, with DR. CHARLES E. PADDOCK, Vice-President of the Chicago Gynecological Society, in the chair.

EXHIBITION OF SPECIMENS

LARGE GUMMA OF THE MESENTERY

DR. ALBERT GOLDSPOHN: We have here an unusually large gumma, much larger than I ever saw in any part of the body, that I was unfortunate enough to be asked to operate upon, and it was upon a mistaken diagnosis. A woman, forty odd years old, who had very marked external evidences of syphilis, applied for operation. Upon seeing her condition, I informed her that she ought to undergo a period of medical treatment before any operation should be attempted. She asked what that treatment would be, and I told her, and she said she would not have any more of that, as it had greatly disturbed her stomach, and that if I did not operate on her, that would settle it. In examining her, I found that she had a movable tumor in the small pelvis, which, from its contour, general characteristics, mobility, relation, and approximation to the uterus, I diagnosed as an ovarian cyst, or a neoplasm of some kind. When we came to operate on the tumor, however, it turned out to be this gumma, which grew in the mesentery of the small intestine pretty high up, and simply hung in the small pelvis. A part of the intestine, whose mesentery had been unfolded by the tumor, was constricted or narrowed; and the woman gave a history of rather unusual constipation. There were no other tumors to be found elsewhere. I could find no tumor or tumors in the liver, or anywhere else in the abdomen, and therefore concluded to take this gumma out, because it could be done. This I did, and made an anastomosis with the Murphy button, and the woman made a normal recovery without any complication or difficulty. She was in a feeble condition for a time, and did not leave our care for about four weeks, and then she was dismissed with strict injunctions to carry on an antisyphilitic, constitutional, medical cure. How long she did that, we do not know. In writing to the husband recently, I learned that she became insane nearly a year after she left the hospital, and some months later died in the asylum.

Microscopic sections have been made of this tumor, and show simply what we would expect in a granulation tumor. If there is any member who desires to see these sections, Dr. Cunningham will exhibit the slides.

MALIGNANT HYPERNEPHROMA

The next specimen is a large hypernephroma which is malignant, as the microscopic characteristics plainly show, and as the clinical history in the case also developed. Microscopic sections of the tumor portion have been made. The microscopic diagnosis was made by ourselves and confirmed in the Zeit Laboratory. The interesting things that we note in this specimen are these grayish-white islands of neoplastic tissue, found in the

sections of both these hemispheres, and particularly this large neoplastic mass at the end, the large end of these sections, which has very clearly crowded aside the normal kidney tissue, forced its way out, and also extended the pathological process to the tissues around the ascending colon, where I found indurations that were probably malignant, but could not be removed without injuring the ascending colon, and a more radical operation was not warranted in that depleted patient. The histological examination of this specimen does not show what we generally look for in hypernephroma, that is, a neoplasm into the renal vein. The authorities say that the metastases occur by the blood current, and that in the renal vein we generally find evidences of the extension. That was not found in this case, but the extension occurred by the tumor crowding aside normal tissue and encroaching upon the connective tissue about the ascending colon.

I may, perhaps, be permitted to allude to the most credited theory, in which the best authorities seem to unite, that the origin of these so-called Grawitz tumors are cellular, or are small remnants or particles of epirenal elements that are aberrant, have strayed into, or have become abnormally placed in the kidney, upon, or beneath, its capsule, and there grow and constitute sometimes malignant tumors, and sometimes not.

TUBOOVARIAN CYSTS

This other collection here is one of five tuboovarian cysts, in which the end of the tube that was severed from the fundus of the uterus is indicated by a pin introduced, and the abdominal end of the tube that enters into the lumen of the cyst proper is indicated by a stick of wood introduced.

The theory of growth of these cysts is also familiar to you. They are not such a remarkable thing any more, as they were some ten years ago. In the beginning, the process is one of inflammation, the result of infection, there being an agglutination of the open fimbriated end of the tube upon the ovary. If such an agglutination occurs firm enough, which we know can easily occur, and it happens that beneath this agglutination there is a follicle cyst, or the beginning of a genuine adenoma, then it can easily happen that the membrane covering that cyst and projecting into the lumen of the tube will perforate, and then we have a coalescence of the tumor cavity and the tube cavity, and the two together will then continue to expand and constitute the cyst wall or cystic contents — originally an inflammatory product, and later continuing to grow because of the continued formation of cystic contents.

EXHIBITION OF INSTRUMENTS

A PUERPERAL CURETTE

I have here a large loop curette, devised from the original French loop curette, such as we use in ordinary gynecological practice for uterine curettage. For a long time I have felt that, in dealing with puerperal cases, general practitioners have not been properly taught as students the difference between curettage in the puerperal uterus and curettage in the gynecological uterus. The ordinary gynecological curette, be it ever so efficient in the ordinary case, is usually a very disastrous and a very dangerous tool to use in the puerperal uterus. It is certainly a very nonsensical affair; it is, to my mind, like going into a bag lined with sheepskin with a buttonhook. Therefore, having such cases to treat and seeing such patients die in the hands of my colleagues (whose cases I have observed) within twenty-four hours after the use of these mischievous, small, ordinary curettes, I studied in regard to this matter; and my practice has been what is probably really the practice of most men, that for removing remnants of the placenta, the ideal curette of first choice is the finger, if it can be introduced, and it can be introduced oftener than one may think, if you will crowd the uterus externally by bimanual palpation down upon the finger in the vagina, and then locate and detach the fragments as well as you can, and remove them after that with a good placental forceps. But oftentimes this removal will be imperfect. There will be something left behind, so that some sort of curette will be needed. The curette should be of large type, not too small, so that it cannot do any injury, if it does not do good. I have here a rather large loop which measures 1⅛ inches by ⅞ inch. It is semi-sharp and has a cannulated handle. Commonly, in curetting a uterus we make wounds that we do not see and which ought not to be made, and we detach septic detritus which is in danger of engrafting itself into these wounds. Therefore, we need a constant current of water to remove the detritus before it can be engrafted into the new atria.

AUTOMATIC PERINEAL RETRACTOR

I show you a perineal retractor which is a modification of the idea that we see in the market of instruments of the same type, with a leaden weight attached to the handle. This weight is an unwelcome thing to handle when we have to handle the instrument, and it is not near enough weight to do any good when we are using the instrument. Then I devised this clamp, forming a gentle arc of a circle, which is attached by one end to the handle of

the retractor and fastened there by a ring slipped upon it. Traction is applied either by hanging a weight in notches upon this arc of a circle, or by tying the latter down with a rubber tube extending to a cross bar between the legs of the operating table. I usually use weights from ordinary large scales, a weight that weighs five pounds, one eight pounds and another ten pounds. If I go some distance, I take a rubber tube with me in place of the weights. The desired amount of tension can be obtained, and the blade of the speculum within he vagina can be given the desired slant, without any danger of dislocation or traction in the wrong direction; so that this important service is attended to entirely by gravitation without any human being's exertion and it leaves the hands of the assistants entirely free for more skillful aid, which is usually needed in major vaginal operations.

There are two of these retractors. One has a wider blade, and spreads the posterior commissure of the vagina more widely apart, and the other narrower. The wider one I use by preference. The other one can be had, however, if someone wishes it.

DISCUSSION ON THE INSTRUMENTS EXHIBITED BY DR. GOLDSPOHN

DR. CHARLES E. PADDOCK: Dr. Goldspohn presents the curette for our consideration, which is used for curetting the puerperal uterus. I can hardly agree with the Doctor as to the efficacy of this instrument. The indications for curetting the puerperal uterus are, I believe, few and far between. If the indication should arise for exploring the interior of the uterus soon after delivery, the finger offers a more successful and, I might say, intelligent means. This treatment is free from danger, and will accomplish all that is desired. The patient can be placed under an anæsthetic and the hand passed into the vagina and one or two fingers passed into the uterus with ease, making counter-pressure at the same time over the fundus with the other hand. I believe the general results would be better, if a curette were never made for this purpose. The danger of increasing infection by removal of the so-called leucocyte zone and the danger of puncturing the uterus by the curette are possibilities, and I might say, probabilities. Hæmorrhage alone might be considered the main indication for a curettement of the puerperal uterus. Again, irrigation at this time is not devoid of its danger, and we frequently find that the fluid has been carried beyond the uterus into and through the tubes.

DR. CARL WAGNER: With regard to the new curette which has been exhibited and described by Dr. Goldspohn, we are all familiar with the comparatively frequent occurrence of perforations and injuries of the uterus by the use of such instruments in the hands of general practitioners. Ohlshausen taught us never to use anything but the finger to clean out the uterus after an abortion.

A few years ago at a meeting of the Chicago Gynecological Society, there were several cases reported of perforations of the uterus with the curette; and some of you may remember that Dr. Herzog was called upon to testify in a case in which the uterus was perforated with such an instrument. We know that the curette is being used constantly by the general practitioner. It seems to me a larger loop is preferable to a smaller one for removing debris from the uterus; and, as I take it that this instrument is designed more for the general practitioner than for the gynecologist or the surgeon, therefore it will be very useful.

A MEMBER: It is not always possible to pass the hand through the vagina. Recently, I had a case where it was impossible to do this, and found that Dr. Goldspohn's curette was an ideal instrument, and when the cervix is sufficiently dilated there is very little chance of forcing the stream of water into the abdominal cavity, as the reflux will always occur, provided the stream is not too strong.

DR. CHANNING W. BARRETT: I wish to commend the instrument exhibited and described by Dr. Goldspohn. The finger is useful for exploring and loosening tissues from the uterus after an abortion, but frequently you cannot remove tissues from the uterus in this manner. The curette that we use for ordinary purposes does not answer at all, as it is too small and is apt to cause perforations. There are different instruments made on the type of the Goldspohn instrument, yet not combining all the features of this. The instrument he presents to-night affords the opportunity of using a douche. I much prefer the uterine irrigator and curette in separate instruments but this can be used, with or without the irrigator, as one prefers. It has a very nice large loop for certain cases, and yet I would think it advisable to have a curette somewhat between that and the ordinary instrument, for I think there would be cases of abortion in which this instrument would enter the cervix with difficulty. Yet this instrument would hardly cause perforations, even though the consistency of the uterus is much less than normal. The flange is made right on this curette. The semi-sharp surface projects beyond the dull surface, and that is an advantage. The semi-sharpness is important — not to be real sharp, not to be real dull. A well-constructed ovum forceps answers the purpose well in cleaning out the uterus in some cases; it fills in the breach

between the finger and the curette; and I should dislike to undertake continuously to do this work of cleaning out the uterus after abortions, without being prepared with all three of these instruments — a trained finger, a well-constructed ovum forceps, and a well-constructed curette.

DR. PADDOCK: In my previous remarks, I meant a uterus that would admit a flat instrument.

DR. GOLDSPOHN (closing the discussion): In regard to the remarks of the chairman (Dr. Paddock), I will simply say that while his principle is correct to a certain degree, yet there are cases, as Dr. Barrett has mentioned, where something else is needed. Now, there are many puerperal cases with beginning sepsis that is no longer altogether within the uterus. There is some infiltration around it, yet from the nature of the discharges from the interior of the uterus, it is evident that there is something left within the uterus that ought to be removed. Then you have a large uterus already beginning to be fixed in its neighborhood and to the tissues round about. If you want to make the finger reach the bottom of that uterus, you must do violence to its bed. You may dislodge thrombi that have already formed in veins in the broad ligaments; and surely you will do vastly more harm, in the absence of this instrument, with the finger to such a uterus and its surroundings, than if you leave the uterus carefully *in situ* and curette. As to liquid passing into the tubes in a case in which the neck of the uterus is large enough to admit this large loop, there is practically no fear or possibility of that, if the uterus is not markedly retroverted. In an extremely retroverted, large, fixed, puerperal, septic uterus, I would doubt the propriety of washing out; but in the absence of marked retroversion of the uterus, there can be no possibility of liquid going into the tubes, even if they are open.

REPORT OF CASES

TOTAL ABSENCE OF INTERNAL GENERATIVE ORGANS

DR. ALBERT GOLDSPOHN: I wish I could demonstrate the woman whose case I am about to report to you, but that is not my privilege. This woman, about forty years of age, was quite stout and excessively fat. Usually she had always been in fair health, and labored for her living. She was married the second time. Her husband, who himself was sexually incompetent, had never found out that his wife had no internal generative organs. Both he and his wife had the idea that she had some tumor, because she had a large pendulous abdomen; and for a number of months she had been having pain in the right side at intervals and subsequently it occurred in regular paroxysms. When I examined the woman for the first time, she had a temperature of 100° in the evening, and evidently had a subacute pelvic peritonitis from some source, which I took to be from a genital source, from the tube or ovary, and therefore ordered the woman to go home and go to bed as much as she could, for at least ten days or two weeks, upon the principle which I hold that pelvic inflammations that come from a genital source, and not from an intestinal or appendix source, should not be operated on in the acute stage; that we should wait until the acute condition has subsided before we operate because pelvic peritonitis from a genital source will practically never become a general peritonitis with ordinary care. After two weeks she returned, with no elevation of temperature, and with less abdominal tenderness. In opening the abdomen, a diseased appendix was found, but there were absolutely no internal generative organs. There were the rectum and bladder as we find in a man, but nothing beyond that. We removed her appendix and that was all. Her external feminine characteristics appeared to be normal.

DR. CHANNING W. BARRETT: I wish to report briefly a case that came to my clinic on the West Side about two years ago. The patient was a young woman, eighteen years of age, who had never menstruated. She had been married two weeks before coming to the clinic, but she was not as fortunate in her choice of a husband as was the case mentioned by Dr. Goldspohn. She was unable to have sexual intercourse, on account of the lack of a vagina. We could find no evidence of internal generative organs, although their absence was not proven by operation. She left the clinic and was charged to come back, but did not. We assume that there was an absence of internal generative organs, only because of the absence of symptoms pointing to menstrual difficulties and the entire absence of a vagina.

A MEMBER: About twenty-five or thirty years ago, a young woman consulted me as to the reason why she did not have her periods. On examination, her external organs were found to be perfect, but there was no evidence of internal generative organs. There was no uterus. I took her to the late Dr. William H. Byford, who examined her carefully, and confirmed my diagnosis. Otherwise, the woman was perfect. She had perfect breasts, good form, and enjoyed good health. She afterwards married, and, so far as I know, lived happily.

DR. CHAS. B. REED then read a paper entitled, Part I. The Conditions Requiring the Removal of the Child from the Breast, and Part II, the Passage of Bacteria from Mother to Child.

DISCUSSION OF DR. REED'S PAPER

DR. F. S. CHURCHILL: I came to learn; I know absolutely nothing about the elimination of drugs through the breast, from any experience of my own. I can speak only from the clinical side. The general impression I have gained from Dr. Reed's paper, without being able to grasp all of the details, is that there have been too few observations on each specific drug for us to be warranted in drawing any conclusions whatever. For example, in chloroform, he reports but one case where the child of a nursing woman who had taken chloroform, became comatose and sleepy. It does not seem that we can draw conclusions from that one instance any more than I could draw conclusions from a case I saw of a baby who, nursing from a morphine fiend, was habitually sleepy, with diminished respiration, pin-hole pupils, and, as far as I know, had not been given any opium by the mouth. My inference was that the baby had been poisoned by elimination of opium from the mother's milk. I took the baby off the breast and it ceased to be comatose, sleepy, and have pin-hole pupils. Yet I do not think I should be justifield in inferring, from that one case, that women generally eliminate opium through the milk.

I should like to ask Dr. Reed if, in these observations, the age of the baby is given and the condition of the mother at the time the milk was examined? It is well known among pediatricians that the breast will vary at different times during lactation, being at times merely a secretory organ, and at other times an excretory organ plus a secretory. When it becomes excretory, it is regarded as being in a state of unstable equilibrium. This condition exists in nursing women the first week, and it will occur at times later, during lactation under varying conditions; it is very apt to occur if the woman is menstruating, or under certain mental conditions. The breast is apparently thrown into a state of unstable equilibrium, the baby is hard to satisfy and generally goes to pieces. In these investigations, do they state the conditions, whether or not the mother was menstruating at the time the drug was given? It seems to me that it would be of practical value to know this, because the chances are we should be much less inclined to give any of the drugs that are injurious to the baby at the time the woman was menstruating than when she was not.

With regard to the experiments on goats: goats are not human beings and probably the influences controlling the production of milk would not act in them just as it does in the woman. We would not be justified in considering that, because a goat excreted these things, a woman would do so.

With regard to "medicating" the child by giving the mother medicine, that strikes me as being exceedingly inaccurate, inexact, and unscientific, and I did not suppose any intelligent physician did it.

In regard to the purely clinical side of the question, I can only say that I feel very sure when certain nursing women eat certain fruits the baby is upset. Why he is upset I do not know. I do not know the scientific explanation, because I have been unable to make any investigations. I know only the fact. A nursing woman must be very careful what sort of fruits she eats, and the same as to certain vegetables that will upset the baby.

There is one interesting phenomenon I cannot forbear speaking of, although it is not exactly germane to the subject. That is the transmission of the Widal reaction through the breast to the baby. It is well known that a baby nursing a woman who has had typhoid and shows the clump reaction, will in its blood give a positive Widal reaction. Consequently if the baby is taken with an obscure illness, and one suspects typhoid, which is rare in the nursing baby, it will be necessary to test the mother's blood, and should it show a positive Widal reaction and should we wish to find how much value the reaction in the baby has, we must first take the baby from the breast. The reaction will disappear from the baby's blood in a day or two after removing it from the nursing woman whose blood gives the reaction.

Now, as to weaning, I think it is a great mistake to take a baby from the breast, except for very good reasons. During the scarlet fever last winter I had a case of scarlet fever in the mother of a four-weeks-old baby. I took the baby off the breast at once. There seemed to be less risk in doing this than in continuing him at the breast. One of my colleagues, however, pursued the opposite course in a similar case, and the baby did not contract the disease. I do not know what subsequently happened to that baby. Mine had more or less digestive trouble through the year then developed pyelonephritis and died. How large a part the digestive disturbance played in that particular case, I do not know.

In regard to menstruation, I think we should be very slow in taking a baby from the breast on that account.

I am still a believer in the analysis of breast milk. I know that some pediatricians attach very little or no importance to it, as we know that milk varies materially during different periods of lactation. One analysis is of no practical value. There should be a number of them, one, say at 10 o'clock in the morning, one at 2 o'clock in the afternoon,

and the other at 6 o'clock in the evening. A mixture of these samples will give us a fair idea of what the milk consists and in what particular direction there is a deficiency, and thus we shall have some guide as to what we ought to do in order to make the necessary correction. For example, I had a baby several years ago, who was doing well, apparently, on the breast, but who stood practically still in her weight for a month. I had the breast milk examined, found it normal as to fats and proteids, but only 5 per cent of sugar, which was far too little for the baby's needs. I figured out the amount of sugar the baby was actually getting at 5 per cent, and what it should have at 7 per cent, fed her the difference in a milk-sugar solution, shortly after which she began to gain in weight. A month or six weeks later, I had another analysis of the breast milk made and found the sugar was 7 per cent. What brought about the change in the mother's milk, I do not know. In the same way, we may find there is a deficiency in fats, and by feeding babies a definite amount of fat, they improve materially. The same may be said with regard to proteids.

Dr Kerley, in his recent text-book, takes a very optimistic view of the future of nursing women. He is of the opinion that the present athletic craze in young girls and young women has brought about much more vigorous women, and that these women are able to nurse their babies longer than the women of eight or ten years ago.

DR. C. S. BACON: I believe it was shown some years ago by Ehrlich and others that a number of toxins and antitoxins were found in the milk, and even the amount of diphtheria antitoxin was determined; I believe it was found to be from one-tenth to one-twentieth of the blood serum; also tetanus antitoxin was found in the milk. Another thing that is definitely known is that the colostrum has a certain amount of disturbing effect on the child. The composition of colostrum is not perfectly understood, and just exactly the time of the appearance and disappearance of the colostrum is not fixed. But I believe we have, in the presence of the colostrum, one of the best rules to guide us as to the danger or lack of danger in any given milk, and that is about as far as we can go. So far as the effect of menstruation, or the effect of pregnancy, or the effect of long lactation is concerned, probably, from the observations that have been made, there is no difference in the milk at all. The milk varies so greatly its composition. In the first place, the methods of examination of milk are so much in dispute that practically no conclusions can be reached; some find much large quantities of albumin and of sugar, and even of fat. Fat, of course, can be determined with the most certainty, but the methods of examination are not by any means agreed upon; and taking a series of observations by the same individual, we find great variations in the milk at different times of the day. It makes all the difference in the world whether it is the first or the last milk in the breast; and the general conclusion must be, that it is impossible by any chemical examination to learn much about the milk, and hence it follows that the common practice of pretending to tell anything about the character of the milk from a single chemical examination is a farce. It seems strange that any one should venture to take a child from the breast because he imagines that the milk does not agree, or because he has had a single chemical examination made in a laboratory, where he does not know what methods are used — an examination that, perhaps, found only two per cent of fat, and because of that the child is taken from the breast. I have sometimes given quite considerable doses of morphine, for example, for after-pains. They are severe and, sometimes, last two or three days. I see no reason why a woman should suffer after a long, hard labor, and lose sleep, and I do not hesitate to give one-fourth of a grain, and perhaps repeat it in the twenty-four hours, and let the child nurse during the administration of the drug; and I have never convinced myself that the child was affected. I have, sometimes, given an anæsthetic, generally ether, for an operation during the period of lactation, and never think of stopping the nursing on account of the operation. My practice is to let the child nurse before the patient is given the anæsthetic, and then perhaps ten or twelve hours later, when the patient is in good condition, and I have never seen the child suffer any. So, I have become skeptical about the poison going from drugs into the milk. Of course, the possibility is there, and it is not irrational to suppose that alkaloids and other substances can pass when toxins or antibodies can pass; but it seems definitely proven that in regard to these two substances, at least, it is an unusual occurrence. As to the effect of any of the ordinary foods on nursing infants, experiments were made in the hospital at Breslau a few years ago, and were reported in detail. For some time the mothers on one side of the ward were given all sorts of foods, including cabbage and pickles, etc., and on the other side of the ward they were fed very carefully. The children, in all cases, were weighed carefully and clinical symptoms exactly recorded. Then an exchange was made, and the patients put on the opposite side of the ward were given the general diet. The conclusion was that

there was absolutely no foundation for the notion that babies are affected by the food. I feel rather positive on that ground, that the ordinary foods have no effect at all upon babies. The observations made by nurses and mothers, are of little value; they jump at conclusions that are absolutely without scientific value. The only question I have in mind before accepting the results given by the essayist to-night, is that I wonder how carefully these observations were made and whether we can rely upon them.

DR. C. E. PADDOCK: As statistics are of value, allow me to report briefly one case of undoubted chloroform poisoning of a new-born baby. About the ninth day following the birth, it became necessary to put the patient under chloroform. Some hours after the operation the infant was put to the breast. It soon fell asleep and with difficulty could it be aroused. It presented every aspect of chloroform poisoning. This lasted for, at least, twenty-four hours. Since this time I have been very particular regarding putting the child to the breast after birth, especially in cases where prolonged anæsthesia has been given during the delivery. I usually leave directions that the child will not be put to the breast for, at least, twenty-four hours following its birth. As there is no doubt that nursing babies can be affected by mother's milk, under no circumstances should the baby be put to the breast for, at least, twenty-four hours after any operation upon the mother where an anæsthetic has been used, and besides this, the milk should be drawn artificially, at least once before the nursing.

DR. WILLIAM J. BUTLER: Perhaps I should not take occasion to speak on this paper, because I did not hear very much of it. However, I wish to utter a note of warning against the tendency to remove the infant from the mother's breast too quickly. The experience of Dr. Reed, which is undoubtedly vast, bears more directly on the maternal side rather than on the side of the infant.

Concerning the removal of the child from the breast on account of some of the infectious diseases mentioned, there are very few conditions that necessitate the removal of the child from the breast of the mother, Active tuberculosis is the most important of these conditions, not only because tubercle bacilli might be excreted through the milk, but to protect the child from tubercular material coughed up by the mother.

Concerning the removal of the child from the breast in other infections mentioned, cases of acute infection of short duration do not require the removal of the child from the breast. In pneumonia, for instance, that is progressing favorably, there is no reason for removing the child from the breast, although he might be nursed less often than usual. Coughing of the mother, from which the child can be protected, is far more important to the infant, I believe, in transmitting the infection than is the milk. In articular rheumatism, I can hardly see how it would be a cause sufficient to remove the child from the mother's breast, unless the mother was so greatly exhausted, or reduced by the disease, as to demand that it be done. In cases of staphylococci infection, I can conceive of no infection of this character sufficient to warrant removing the child from the breast, unless it be a mastitis which involves the entire breast. Even then, the child could be nursed by the opposite breast. There are few reasons for removing the infant from the breast in the first weeks, when it is so important to the future of the infant. Scarlet fever and even erysipelas, provided the latter does not involve the breasts, are not indications for removing a child from the breast.

In all these infections the infant has the advantage of the immunizing apparatus of its mother, as well as its own. I regard the dangers of contagion from infections in the mother, which are not alarmingly frequent, as of far less import to the infant in the first weeks or months of life, than the inevitable dangers of artificial feeding at this early age.

DR. CHANNING W. BARRETT: One very important thing that should take place in the mother, after the birth of the child, is the involution of the pelvic organs. If the child is taken immediately off the breast, there is a certain amount of subinvolution following. Nursing the child aids involution of the pelvic organs, even to hyperinvolution, thus giving the pelvic organs a certain period of rest which is advantageous, from these considerations. This should be take into consideration when the advantages and disadvantages to the mother are being weighed.

DR. GUSTAV KOLISCHER: I think Dr. Reed's paper is a most appropriate one for a meeting of this kind, because he has given us the present status of a very important question. The paper shows evidences of careful preparation along these lines, and as the evidence presented has been sifted by a critical mind, it is necessarily a distinct and valuable step forward. It is an advance in one of the most important branches of medicine — preventive medicine.

DR. EFFA V. DAVIS: I think the essayist might have given us a more hopeful outlook when suckling is discontinued in the presence of acute diseases. For instance, in mild cases of pneumonia, where the disease lasts but a short time, nursing should be resumed as soon as the disease has subsided.

I desire to emphasize one point, that there are many physicians who do not realize how much they are injuring the mother by putting the child at the breast again, following some diseases, because these mothers have not seen the last of their troubles when they have ceased to nurse their babies. Some of them are invalids for many months afterwards.

DR. CHARLES B. REED, in closing, said:

In reply to Dr. Churchill, I would say that the age of the baby was not taken, so far as I could make out. The cases I have quoted are not clinical cases, and for that reason I expected to bring out more clinical cases in the discussion to-night. The cases reported were apparently scientifically demonstrated. The literature abounds in clinical reports, but as Dr. Bacon states, they are not scientifically observed. I was greatly astonished to find such cases are extremely rare. Where I was not confident the observer had gone carefully over the ground, I referred to them casually and emphasized that they were not as yet scientifically demonstrated. The literature is extremely barren of cases of that kind, although abundant in clinical cases. Of course I agree with Dr. Churchill in regard to the value of animal experiments. They are valuable only as a means of more or less imperfect education as to what might happen in the human family; and I willingly concede how little the value they possess.

I regarded the chief value of my paper to be in the discussion that it might elicit. I have been very much astonished to find how little literature we have on this subject in English, and that the French and Italians are the only men who have studied this work to any great extent; and I confess my own ignorance was the starting-point for the working out this paper which has been a source of extreme interest.

I find myself quite in accord with the gentlemen who have criticized the paper, in almost every respect. It is, of course, the ideal to keep the child at the breast whenever possible; but the object of the paper is to call attention to some conditions which, in my opinion, should or may require the removal of the babe. The remarks of Dr. Churchill were cogent and to the point. I think possibly he emphasized, a little more definitely than I intended to do, the withdrawal of the baby during the menstrual flow. It was not the intention that the child should be removed permanently from the breast on account of the difficulties arising during the menstrual flow, but that when symptoms pointing to intestinal difficulty appeared, and when they could not be traced to anything else, and could be coördinated in time possibly, and in other respects with the menstrual flow, it was the part of prevision, at any rate, to remove the child until it should be able to nurse a healthy breast or take the unintoxicated milk, if I may use that term.

The constantly increasing weight of the child is the great desideratum; and the presence of intestinal difficulties of one kind or another, showing infection, irritation, indigestion, or similar conditions, is simply a signal for an accurate investigation of the reasons for such a change, and the removal of the child, if it can be traced either to the mother or to some condition which the mother presents; and, of course, involves the immediate restoration of the child to the breast when such a condition disappears. On the contrary, I cannot endorse the opinion that has been expressed to-night of using the baby merely as a medicament, merely as a means for restoring the mother to normal health, or for obtaining a desirable change thereby in the mother, when conditions exist that will by such an act imperil the life of the child. For instance, if we have a serious puerperal infection, with the consequent bacteriæmia which must invariably be present, I would regard it as highly criminal to put a baby to the breast for the purpose of securing thereby the much-desired involution of that organ. Neither do I believe it would be logical to produce any other organic change in the mother at the expense of the child, under such circumstances or by such means. The child has now become a definite entity; it is entitled to just as much consideration as the mother, and this consideration must be accorded to it, and its protection must be just as marked and just as well observed as that of the mother. I do not think we are justified in imperiling the child to the least degree for the sake of the mother. I do not believe Dr. Barrett meant, in cases where bacteria float in the blood, or in any of those infections in which the blood is necessarily filled with bacteria, that the child should be used for such a purpose.

DR. GUSTAV KOLISCHER read a paper entitled, Successful Treatment of Chronic Gonorrhœal Endometritis.

DISCUSSION

DR. ALBERT GOLDSPOHN: I would like to ask Dr. Kolischer as to the strength of the solution he uses on the intrauterine tampon.

DR. KOLISCHER (closing the discussion): I think I mentioned in my paper that I use three per cent albargin.

Albargin is a so-called organic silver salt which produces a strong reaction without causing much pain.

ABSTRACTS OF CURRENT LITERATURE

DEPARTMENT OF SURGERY

UNDER THE CHARGE OF

JOHN B. MURPHY, M. D.
FREDERIC A. BESLEY, M. D.
WM. C. DANFORTH, M. D.
AIMÉ PAUL HEINECK, M. D.
E. WYLLYS ANDREWS, M. D.
WM. R. CUBBINS, M. D.
M. G. SEELIG, M. D.
GEO. E. BEILBY, M. D.

JUXTA-INTESTINAL AND JUXTA-UMBILICAL ENTEROID CYSTS. By Ch. Bouveret.

Under the name of enteroid cysts, we designate cysts the histological structure of which recalls that of the intestine. If these cysts be near the abdominal wall and be remnants of the omphalomesenteric duct, we call them juxta-umbilical enteroid cysts. If they be in the neighborhood of the intestine, we call them juxta-enteroid cysts.

These cysts are congenital and their development is intimately connected with the involution of the omphalomesenteric canal. We are dealing with the isolated and encysted debris of this duct. They co-exist at times with other congenital malformations. These cysts are almost always unilocular, not very voluminous, rarely exceed the volume of an orange. They contain a viscous fluid, clear or brownish-yellow in color; this fluid is at times hemorrhagic, and contains in suspension fat and cholesterin. In relation to the intestine, they are submucous, intramuscular or subserous. In the last case, they may be seated at the free border of the intestine or at its mesenteric border, separating the two folds of the mesentery. Their histological structure is very analagous to that of the intestinal wall, with some differences nevertheless. The wall is very thick and is lined with cylindrical epithelium. There are no true glands of LieberKühn, but there are irregular and ramified glanduliform crypts. At times, these crypts are exuberant enough to give to the wall an adenomatous aspect. It contains no lymphoid formations.

From the clinical standpoint, these cysts are observed especially in early childhood, and though congenital, they may reveal their existence but at advanced age. They present only vague symptoms and it is only the occasion of complications such as an intestinal occlusion that leads to their discovery. In some cases, vomiting occurs and by examination one can feel a regular, not very voluminous, tumor, the characteristics of which are not very specific.

The diagnosis of these cysts is very difficult. So far, it has never been made before opening the abdominal wall. We must think of the possibility of an enterocystoma in cases of acute intestinal occlusion that occur a few days after birth, especially if the subject be the bearer of other malformations.

The prognosis of these cysts is serious, owing to the frequency of the complications. The treatment has nothing particular. Most always, the indication is an operation for volvulus or drainage with extirpation of the cyst.— *Kystes enteroides juxta-intestinaux et juxta-ombilicaux. Archives générales de Chirurgie.* 25 janvier, 1908.

AIMÉ PAUL HEINECK.

THYPHOID CHOLECYSTITIS. By Khautz.

Owing to symptoms of cholelithiasis, most cases of typhoidal cholecystitis are operated long after the disappearance of the typhoid fever. Only rarely is it necessary to operate in the course of typhoid fever.

From the clinical standpoint, typhoid cholecystitis may be classified into two categories: 1. The cases which are diagnosed in the course of general infection, and 2. the cases which appear long after the disappearance of all typhoidal symptoms.

In the first, as a general rule, one finds no calculi, but only an emphyema of the gall-bladder. In the second, calculi are never missing. Cultures of the contents of the gall-bladder show typhoid bacilli long after the infection, even ten years after. The history is obscure, and often throws no light upon the path of the infection. The obscurity of the symptoms is shown by the following case:

A woman, 34 years of age, without a history of typhoid fever, showed all the symptoms of an acute suppurative cholecystitis. At operation, we found the inflammed gall-bladder filled with brown-

ish, serous, odorless fluid and containing many calculi. A cystectomy was performed and the next day there was a critical fall of temperature. Cure followed. The bacteriological examination of the contents of the gall-bladder showed a pure culture of typhoid bacillus. Eighteen days after operation, the Widal reaction was positive.— *Sur la cholécystite typhique. Archives générales de Chirurgie.* 25 janvier, 1908.

INGUINAL HERNIAS SAID TO HAVE LOST THEIR RIGHT OF RESIDENCE. By Legrand.

In all individuals having a hernia of this nature, there is a diminution of the size of the peritoneal cavity and a tendency to general ptosis of the viscera. These hernias, despite their extraordinary volume, are not associated usually with marked functional disturbances. Their complications, however, may imperil the lives of their bearer. These hernias, as all other hernias, should be operated. However, before attempting to make a radical cure, attempts to return and to keep the intestines within the abdominal cavity should be made. Only exceptionally is it permissible to resect the intestines.

For the rebuilding of the abdominal wall, the preferred method is Bassini's method. But in these cases, the hernial rings are so patulous that it is very difficult to follow rigorously any stereotyped method. In cases where the abdominal wall is unusually weakened and where we very much fear a recurrence, the complete closure of the inguinal canal can be proposed to the patient. After the operation, the patient must wear for a long period an abdominal binder, with a pad for support of the cicatrix. Counterindications to operative intervention are few. Therefore, most of these hernias can be returned to the abdominal cavity, and their right of residence therein regained. — *Des hernies inguinales dites ayant perdu droit de domicile. Archives générales de Chirurgie.* 25 janvier, 1908. AIMÉ PAUL HEINECK.

STERILIZATION OF METAL INSTRUMENTS WITHOUT RUSTING.

Joseph Levai calls attention to the fact that steel instruments rust as a result of coming in contact with oxygen, carbon dioxide and moisture. If one of these three factors be absent, rusting cannot occur. The iron of the instrument combines with oxygen and carbon dioxide to form ferrous carbonate; and this ferrous carbonate, in the presence of moisture, is converted into ferrous oxide or rust. The principle upon which Levai proposes to avoid rusting rests on the fact that all carbon dioxide is absorbed by the addition of sodium hydroxide (Na O. H.) to the water in which the instruments are boiled. He recommends a ¼ per cent solution and reports that for over 10 years he has used this method, and that during this time he has never been bothered by the rusting of instruments. Moreover, the solution does not in any way impair the cutting edge of instruments. Sodium carbonate, which is commonly used, does not accomplish the same purpose as does sodium hydroxide, for the reason that it does not combine with carbon dioxide. The lightest coat of nickeling is in no way affected by the hydroxide solution. It is essential that the solution should not be stronger than ¼ per cent, in order that the instrument be not rendered slippery.— *Cent. f. Chir.*, No. 5, 1908. M. G. SEELIG.

EXPERIENCES IN OPERATING FOR ABSCESS AND GANGRENE OF THE LUNG.

Professor W. Korte, of Berlin, in this lengthy paper details his experiences in 58 cases of surgical pulmonary disease. The article adds an almost complete chapter to the subject of lung surgery, and takes up in detail questions of etiology, physical signs, diagnosis and operative technique. As a result of his clinical experience, Korte concludes that acute pulmonary suppuration should be treated medically for a few weeks, unless the process is a very putrid one. If despite this treatment, temperature persists and the disease progresses, a pneumotomy should be performed. As a rule the prognosis, after pneumotomy is good.

In cases of bronchiectatic cavities, an operation should be performed only if the cavities are confined to one lobe. In these cases the operative procedure must be very radical:— wide resection of the chest wall, extensive splitting of the lung tissue thoroughly to expose the cavity, or even resection of a lobe of the lung.

It still remains to be settled whether or not the operative mortality is improved by operating in a negative pressure chamber.

A fairly comprehensive casuistic report of all the cases operated upon is appended to the paper. — *Arch. f. Klin. Chir.*, b. 85, h. 1.

M. G. SEELIG.

THE PATHOLOGY AND TREATMENT OF THE SO-CALLED BANTIS' DISEASE. By Dr. F. R. Nager and Dr. J. Baümlin from the Canton Hospital in Münsterlingen. Director, Dr. Conrad Brunner.

These writers report a case of Banti's Disease in a young woman of 25 years, operated and the spleen removed by Brunner with a successful recovery, the patient being in good health two years

after operation. Examination of the spleen removed in this case, showed a high degree of hyperplasia of the spleen stroma, or an indurative splenitis. A marked degree of vessel change, consisting in sclerosis and circumvascular formation of connective tissue, was present, leading to atrophy of the follicles. Hemorrhages into the tissue and scars of old infarcts were found and many giant cells were present. Eosinaphiles were plentifully present.

As to symptomatology, the first and most marked indication of the disease is the progressive enlargement of the spleen. This may last for years before other symptoms, particularly a particular form of anæmia appears. The blood findings are of particular importance. The results of Nager and Baümlin's work along this line do not vary from those of standard authors already in print.

As to treatment the writers are of the opinion that the only correct treatment is splenectomy, as the prognosis is good and the result permanent. They emphasize that the spleen should be removed before the liver complications become manifest.

As to the operative prognosis, fourteen cases are quoted from the literature, operated by various surgeons, of which 10 recovered. Three of the 4 fatal cases died of hemorrhage. Comparing these results with other statistics a total mortality of reported cases of 20 to 30 per cent is quoted.

The most important points are summed up as follows:

1. Clinical observations and histological findings indicate that Banti's Disease is to be looked upon as a clinical entity, and as a primary disease of the spleen.

2. A careful differential diagnosis rules out many mistakes, many of which are shown in the cases published up to now. In particular are hereditary lues and hepatic cirrhosis to be separated from Banti's disease.

3. The first or anæmic stage of Banti's disease is identical with primitive splenomegaly and splenic anæmia.

4. Splenectomy is the only correct therapy for true cases of splenomegaly with liver cirrhosis. To this may be added, in the ascitic stage, a Talma operation.— *Beitrag zur Pathologie und Therapie de sogenannter Banti's-schen Krankheit Beiträge zur klinischen Chirurgie,* vol lxv.

W. C. DANFORTH.

GLYCOSURIA AS A CONSEQUENCE OF ABDOMINAL TUMORS. By Wilhelm Evelt of Munich.

Evelt reports a case of glycosuria which he considers due to the pressure of an abdominal tumor on the pancreatic duct.

The patient, a woman of 41, had a multinodular myomatous uterus, reaching nearly to the umbilicus, and a large multilocular cyst of the left ovary. Before operation the urinalysis showed the presence of albumin and quite an amount of sugar. After the removal of the uterus and the ovarian tumor, no albumin or sugar was found at any urine examination after the fourth day. After the seventh day she had regular ward diet. She was under observation for one year afterward and neither sugar nor albumin was ever found in the urine.

Evelt has found but one similar case reported. Fellner reported in the *Wiener Klinischen Wochenschrift* a case of double pyosalpinx in which sugar present before operation disappeared afterward.

In explaining the phenomenon Evelt states that diabetes is out of the question, in as much as the glycosuria was transient. After review of the known or hypothetical causes of glycosuria, we must conclude that an apparently abnormal activity of the pancreas is the cause in this case. Atrophy of the pancreas cannot explain it, in as much as an atrophied pancreas could not become normal again in four days. The rapid disappearance of the sugar after operation seems to point to a mechanical cause of the glycosuria. Compression of the pancreatic duct is the most likely mechanical cause. The tumors which filled the entire abdominal cavity were sufficient to exert such pressure on the duct; and this is the explanation offered.

It is granted that ligation of the pancreatic duct in animals has never caused glycosuria, but too close analogy must not be drawn between animals and men. Moreover, in 1906, Marchand described a case in which glycosuria was apparently caused by stones impacted in the pancreatic duct.

This case emphasizes the care which must be taken in examining a patient with glycosuria. A small tumor compressing the duct and giving in itself no symptoms may cause a diagnosis of diabetes to be made, the tumor itself being entirely overlooked. Therefore, in all cases of glycosuria the region of the pancreas should be carefully examined.— *Monatsschrift für Geburtshülfe und Gynäkologie,* bd. xxvi, h. 6, Dez., 07.

GERRY R. HOLDEN.

JEJUNOSTOMY. By J. L. David.

In cancer of the stomach, jejunostomy is a palliative operation of secondary value. It is only performed when gasterectomy and gastro-enterostomy are counterindicated and can give only mediocre results. In peptic ulcer, its indications are more numerous. The uncomplicated non-pyloric ulcer, if it resists medical treatment, is beneficially influenced in its course by the creation of a jejunal mouth. In burns of the œsophagus, of the stomach etc., jejunostomy is the operation of choice. It has

given good results and can give better ones. Two operative procedures give excellent results:

1. Procédé en Y avec torsion à la Souligoux ou variantes de Quénu, de Kelling.

2. Procédé de Eiselsberg-Witzel.

The former operation, more complicated, is for patients that have still much vitality. The latter, simpler, more rapid, less grave, is adapted to weakened patients incapable of supporting an intervention of some duration.

Jejunostomy is permanent or temporary. If temporary, the fistula must be maintained a relatively long time even if the improvement is noticeably very rapid. At first, much care must be shown in feeding the patient through the natural channels. Before beginning to feed the patient through the natural channels, it is indispensable to determine whether the gastric chemistry has returned to the normal.— *De la jejunostomie. Archives générales de Chirurgie,* 25 janvier, 1908. AIMÉ PAUL HEINECK.

EXPERIMENTAL SURGERY OF THE LUNGS. (A Preliminary Report). By Samuel Robinson.

Successful operations on the pleural cavity demand at least one factor — the avoidance of lung collapse. The effect of lung collapse is a series of circulatory, nervous and respiratory disturbances which, if collapse is continued, result in death. Collapse must be avoided in one of two ways. Either air must not be allowed to enter between the pleural layers, or the lung must be kept artificially inflated. The former is the keynote of the older methods. Advantage has been taken of the existence of inflammatory adhesions for this purpose. In other cases, adhesions have been produced by the injections of irritating substances into the pleural cavity, or by suturing. The infrequency of intra-thoracic operations indicates the danger associated with the use of these methods. The prevention of collapse by means of withdrawing a lobe, is unsatisfactory, since the field is obstructed and the irritation of the vagus terminals causes respiratory and cardiac disturbances. Until exploratory pleurotomy and intra-thoracic inspection become thoroughly safe procedures, the advance of intra-pleural surgery must be restricted.

The second method, that of artificial inflation, is making rapid progress. This method may be carried out by means of external negative pressure or by internal positive pressure. Sauerbruch, objecting to the latter principle, constructed the negative pressure cabinet. This, on account of its costliness and weight, is not adaptable to private and general use.

Robinson undertook this line of investigation in order to perfect a suitable positive pressure apparatus and to test the objections of Sauerbruch on the basis of practical surgical value, and if the apparatus should prove satisfactory, to note further the effect of positive pressure inflation on blood-pressure and on the cardiac and respiratory functions.

The apparatus constructed consists of a glass cone, provided with a rubber collar, which fits closely around the dog's muzzle. This cone has an inlet for compressed air, which may be mixed with ether in any proportion desired by the operator, and an outlet tube, connected with a water-pressure bottle. On both inlet and outlet tubes are manometers, showing pressure being used. The apparatus is satisfactory for experimental thoracic surgery and is easily controlled by the operator.

Thirty animal experiments were done, ranging from simple pleurotomy to excision of the entire right lung, with 9 deaths and 21 recoveries. Of the deaths, 4 were caused by sepsis and one by leakage. The other 4 deaths occurred in those in which a lobe or the entire right lung was excised. The fatal terminations in these last named cases are hard to explain, but were probably due to the existence of a cavity in which the pressure did not equal that of the unopened side, interfering in this way with the heart action. Removal of an entire lobe was done successfully on several animals.

No ill effects, either immediate or late, were noted from the positive pressure. No emphysema was found at autopsy. The cause of the pleuritic effusion following one-sided pneumectomy, will be investigated further.— *Annals of Surgery.* Feb., 1908. Vol. xivii, p. 184.

STENOSING ULCER OF THE DUODENUM. By Ch. Gautier.

As is usual, the precise diagnosis had not been established. Symptoms were those of ulcer of the pylorus. There was no vomiting of bile, no intestinal hemorrhages. These signs have been given as characteristic of duodenal ulcer. The patient was laparotomized and an ulcer of the second portion of the duodenum was found, to which was adherent the fundus of the gall-bladder. The ulcer was firmly bound to the pancreas. We did not practice an extirpation, we performed a simple gastro-enterostomy. The vomiting disappeared, but the low digestive pains occurring two or three hours after meals, were not cured. These pains must be attributed to the passage of the acid chyme of the stomach upon the duodenal lesion.

The operation which we should have performed is exclusion of the pylorus. The patient declined to submit to another operation.— *Ulcère sténosant du duodénum. Archives générales de Chirurgie.* 25 janvier, 1908.

JEJUNAL PEPTIC ULCER. By P. Vachez.

Peptic ulcer of the jejunum is not an uncommon affection after gastro-enterostomies. The author has been able to collect 36 observations, of which six are personal. From the anatomical standpoint, jejunal ulcer differs but little from gastric or duodenal ulcer. In its acute form, the ulcer rapidly leads to perforation before the peritoneum has had time to react; in the chronic form, which is the most frequent where the intestinal wall is infiltrated, it is callous and there are visceral parietal adhesions. Almost always the ulcer is single and is seated upon the descending branch of the anastomosis.

The etiology of the jejunal ulcer has been much discussed. The author enumerates the different theories advanced and believes that the stasis of hyperacid gastric juice is the abdominal cause of ulcerating jejunitis. The operative technique is perhaps an etiological factor. Anterior gastro-enterostomy favors stasis and thus ulcer formation. Roux Y method of posterior gastro-enterostomy is directed against this stasis and prevents the acid chyme from long sojourning in the jejunum.

Clinically we have two forms: The latent form, with sudden perforation, which gives as an intensely acute peritonitis, always fatal; then we have the chronic adhesive form which is characterized by gastro-intestinal disturbances with peritoneal reaction. This peritoneal reaction manifests itself by the formation of a thickened adherent mass. The diagnosis is fairly easy. Individuals upon whom gastro-enterostomy has been performed, should be attentively watched. The preventive treatment should consist in an appropriate diet to which the alkalies have been added. Once the ulcer is established, surgical treatment should be instituted. The ulcer will be excised and a new opening further from the plyorus must be created.— *L'ulcère peptique du jejunum. Archives générales de Chirurgie.* 25 janvier, 1908.

STENOSES OF TYPHOIDAL ORIGIN LOCATED IN THE SMALL INTESTINE. By Ménon Paul.

These may be divided into two main varieties: The occlusions that occur during the course of typhoid fever and in the first month; second, the late occlusions which appear after the first month.

Among the early occlusions, the most frequent form is the invagination; then comes coprostasis and intestinal paresis of such nature as to necessitate enterostomy.

Far more frequent are the late occlusions. They are ordinarily caused by a typhoidal peritonitis, due either to propagation or to ulceration. In a few cases that have been very much discussed, it has been thought the occlusions were secondary to the cicatrization of the typhoidal ulceration, but stenosis following typhoid fever is rare. Stenosis following tuberculosis of the small intestine is very frequent compared with stenosis after typhoid fever.

Peritonitis acts through the mechanism of adhesions, which may be firm or loose depending upon the age of the peritoneal accidents. This peritonitis produces angulations, twists. At times, it is associated with suppurative mesenteric lymphadenitis. Therapeutically, we will act as follows: If the adhesions be recent, it will suffice to separate them. If they be old, extensive, placed in a dangerous region, it will be advantageous to establish an enteroanastomosis between a loop of gut above and a loop of gut below. The other causes of occlusions will be treated according to their nature. The more recent the lesions, the better the prognosis.— *Des sténoses de l'intestin grêle d'origine typhique. Archives générales de Chirurgie.* 25 janvier, 1908.

A CONTRIBUTION TO THE STUDY OF RUPTURE OF THE LIVER. By Von Kippel.

The author reports a case of subcutaneous injury to the right lobe of the liver. The patient had fallen from his horse and his abdomen had borne the weight of the horse and of the saddle. Diagnosis of internal hemorrhage was made. After a minute exploration of the abdominal cavity, a bleeding wound of the right lobe of the liver was seen. The hemorrhage was stopped by tamponade; the patient recovered.

The author discusses the symptomatology of wounds of the liver. Usually, the diagnosis is to be made only after the abdominal wall has been opened, and then it is made by seeing the seat of the hemorrhage.

As to the operative technique to follow, we are still in doubt as to which way is better, to suture these wounds of the liver, or to pack them. The author prefers packing. He thinks it is more rapid, less difficult, and more reliable. The liver must be compressed from below, up towards the diaphragm. Only iodoform gauze should be used.

At the conclusion of his article, the author has collected all the cases of wounds of the liver reported previous to 1905.— *Archives Générale de Chir.* Paris, Dec., 1907.